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Rannveig K. Hart, Synøve N. Andersen and Nina Drange

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Rannveig K. Hart, Synøve N. Andersen and Nina Drange

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Abstract:
Long paternity leaves have the potential for lasting effects on parental unions, potentially reducing specialization and increasing union stability and fertility. We put these hypotheses to a causal test, using an extension of the Norwegian parental leave father's quota from 6 to 10 weeks as a source of exogenous variation in fathers' leave uptake. We implement a Regression Discontinuity design, using full population data from Norwegian administrative registers of parents of children in a four-month window around the reform (N = 9 757). The reform significantly increased the amount of leave taken by fathers by about three weeks and reduced the amount of leave taken by mothers. Neither union stability, fertility nor his or her subsequent earnings were affected by the reform.

Keywords: Union dissolution, father involvement, quasi experiment

JEL classification: J12, J13, J16, J18

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Address: Rannveig K. Hart, Statistics Norway, Research Department. E-mail: rvk@ssb.no

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Sammendrag

Økt fedrekvote kan føre til varige endringer i spesialisering, samlivsstabilitet og videre barnefødsler for par med barn. Vi undersøker denne hypotesen i et kvasieksperimentelt design. Vi bruker en utvidelse av den norske fedrekvoten fra 6 til 10 uker, innført i 2009, som kilde til tilfeldig variasjon i permisjonslengde. Data er hentet fra administrative registre, og inkluderer par som fikk barn i et vindu på fire måneder rundt implementeringsdatoen 1. juli (N=9 757). Ved hjelp av et regresjons-diskonituitetsdesign sammenlikner vi par som fikk barn rett før og rett etter at reformen ble implementert. Logikken bak et slikt design er at det ikke finnes systematiske forskjeller mellom parene som fikk barn rett før og rett etter reformen, og at eventuelle endringer i spesialiseringen, samlivsstabiliteten og/eller videre barnefødsler derfor vil være drevet av forskjelellen i permisjonslengde og ikke av andre egenskaper ved parene.

Resultatene viser at reformen gir en umiddelbar økning i fedres permisjonslengde på omtrent tre uker, mens mødre reduserer sin permisjonslengde. Dette er en betydelig endring i permisjonsuttak som gjør det mulig å studere effekter av lengre pappapermisjon på andre utfall.

Til tross for denne endringen finner vi at verken hennes heller hans inntekt de neste årene blir signifikant påvirket av reformen, og det gjør heller ikke samlivsstabilitet og barnefødsler. Dette gjelder for både samboende of gifte par. Våre funn går derfor inn i en litteratur som jevnt over viser få effekter av endringer i fedrekvotens lengde.

1 Introduction

Newer theoretical contributions have suggested that a deficit of gender equality in families can reduce family well-being and, in turn, union stability and fertility (Goldscheider et al., 2015; Cooke, 2006; Esping-Andersen and Billari, 2015). The proposed mechanism is that women suffer under a "double burden" of paid and unpaid work, and when this burden is alleviated, both union stability and parity progression may recuperate. This proposition is corroborated by studies showing that lower father involvement is associated with lower female relationship satisfaction (Kaufman, 2000; Barstad, 2014) and lower union stability (Ruppanner et al., 2017; Sigle-Rushton, 2010; Amato, 2007) . Specifically, in Norway, Sweden and Iceland, longer paternity leave correlates with both higher union stability (Lappegård et al., 2019) and higher risks of second births (Duvander et al., 2016a). As fathers who are more committed to their partner may both spend more time on care work and be likely to want another child, these associations need not indicate a causal relationship.

Gender equal and stable parental unions, as well as fertility levels hindering rapid population decline, are considered politically desired. While the division of unpaid work is outside the realm of state regulations, state compensated paternity leave may constitute a rare opportunity for policy influence. Many countries have policies in place that incentivize fathers' participation in paid parental leave programs, often referred to as a "father quotas" or "daddy quotas" (see e.g. Patnaik (2016) for an overview). The introduction of paternity quotas effectively increase both the share of fathers taking leave and the number of leave days taken by fathers (cf. Cools et al. 2015 for Norway; Ekberg et al. 2013 for Sweden; Geisler and Kreyenfeld 2012 for Germany; Patnaik 2016 for Canada). There is no consistent evidence that such quotas equalize earnings, yet our knowledge on their importance for demographic processes remain scarce.

This paper extends our knowledge on effects of paternity quotas by aiming to causally establish whether a government-induced extension of an existing paternity quota has consequences for the union stability and fertility of couples affected. We take advantage of an extension of the Norwegian paternity quota from 6 to 10 weeks, which took effect for parents of children born July 1, 2009 and onwards. The reform added two weeks to the total parental leave period, and shifted two weeks from the shared period to the period reserved for the father (NAV, 2015b)[1], incentivizing fathers to increase their time at home by four weeks and mothers to decrease their time at home by two weeks.

We expand upon previous literature in two main ways. First, most previous studies focus on introdution of quotas, to which immediate response is typically a minority behavior. For instance, the 1993 introduction in Norway raised uptake from 3 to 25 per cent, meaning that leave-taking remained a minority behavior among fathers also (immediately) after the introduction (Cools et al., 2015). In contrast, 3 in 4 fathers were already taking some parental leave at the time of the 2009 extension (Fougner, 2012). This "normalization" of paternity leave could potentially facilitate a larger immediate reform response, and subsequently lead to more profound changes in parents' behavior than had we only observed families with the most dedicated fathers. A growing literature on effects of introduction of paternity qoutas cannot tell us whether such non-linear responses exist. Qualitative studies indicate that fathers who take paternity quotas of this length indeed spend most of their time caring for their child alone, and that they consider this a learning experience that permantently strengthens the bond between themselves and their child (Østbakken et al., 2018).

Regarding outcomes, effects of (the introduction of) paternity quotas on market work are by now well documented. Of equal importance, yet substantially less studied, is the effect of paternity leave on demographic processes. While gender equality is repeatedly suggested to be important for fertility, empirical tests of this proposition with good causal identification is remarkably rare. The potential of long paternity quotas in stabilizing parental unions is also clearly understudied, particularly with respect to the statistically less stable cohabiting unions. Cools et al. (2015) find no significant effects of the introduction of the four-week paternity quota in Norway in 1993 on marital stability when the focal child is 14 years old. In contrast, Avdic and Karimi (2018) find that the introduction of the Swedish paternity quota reduced union stability among low-earning couples. We expand upon the previous Norwegian study both by including the more fragile cohabiting unions (Hart et al., 2017; Lyngstad and Jalovaara, 2010), potentially more easily moved, and by investigating if a paternity leave of longer duration has more profound effects on union stability. Regarding fertility, to the best of our knowledge, the only example of a causal design to identify effects of father involvement on fertility to date is Cools et al. (2015). They find that the introduction of the Norwegian paternity quota has no significant effects on the probability of having another joint child. In contrast, Farré and González (2017) utilize the implementation of a two-weeks father's quota in Spain in a regression discontinuity design and find that (short) paternity leave delay childbearing and reduce higher-order births among women above 35.

To estimate the effect of the extension of the father's quota on leave uptake, union stability and fertility we employ a Regression Discontinuity design – an identification strategy very unlikely to be biased by gradual changes in fathering practices over time. We compare couples with children born just before the extension of the father's quota to couples who had a child just after this date. For precise estimation of effects for these relatively small subgroups, the sample size provided by full population data is crucial. Our main study sample consists of 9 757 parental couples who were coresiding prior to pregnancy, and where the mother was employed the year before the focal child was born (excluding most mothers not eligible for paid parental leave). We test empirically whether the reform did in fact change the pattern of parental leave uptake for mothers and fathers. All outcome variables are drawn from administrative registers, ensuring zero attrition and high validity. Binwidth is estimated empirically in a local linear regression, yet constrained to be equal at each side of the cutoff. To better understand mechanisms driving effects (or the absence thereof), we also estimate effects on the labor supply of fathers and mothers at the intensive and extensive margin, as well as her share of couple earnings. A decrease in fathers' earnings can be (and often is) interpreted as a(nother) sign of his increased efforts in unpaid work, and hence a desired policy outcome given the underlying goal of increasing gender equality. However, we note that a negative effect on his earnings also will emerge if extended paternity leave signals lower work commitment, leading to subsequent wage discrimination, i.e. intensified fatherhood wage penalties.

The results show that the reform induced fathers to immediately increase their leave length by three weeks. Mothers significantly reduced their number of leave days with up to 21 days, yet the exact length of this reduction is somewhat sensitive to specification. Despite this considerable change in leave uptake, we do, however, not find significant effects on union stability or subsequent fertility. Measures of market work are also unmoved. A battery of robustness checks, including a placebo reform, supports a causal interpretation of our results. Our results yield little support to the potential of policy-induced father involvement to stabilize unions and increase fertility. As such, they accentuate the potential importance of selection in producing the previously observed associations between father involvement on one side, and union stability and high fertility on the other.

Our results have important implications for policy. They do not support the notion of larger and more stable families as a welcome, yet unintended side effect of paternity quotas. To the extent that paternal involvement in other forms is expected to affect demographic outcomes, our results call for a careful evaulation on such expectations in a plausibly causal design. Equally important, however, our results suggests that fathers who were moved by the reform to stay out of the labor market for an extended period of time experienced no (increased) fatherhood penalty. Our results indicate that fathers may take prolonged work extensions to care for their newborn, without fear of facing subsequent earnings penalties.

2 Theoretical framework and previous research

Across Western societies, the birth of a(nother) child fuels gender specialization. Becker (1991, p. 39) suggests that this process is driven by women's small biological comparative advantage in nursing and care for newborns, and there is ample evidence that gendered expectations and norms play a role in manifesting and strengthening specialization (Bittman et al., 2003; Brines, 1994; Hochschild and Machung, 2012; Ono and Raymo, 2006; West and Zimmerman, 1987). While the negative consequences of specialization for women's career and economic independence are obvious, specialization was long expected to increase the gain from marriage and both stabilize unions and increase fertility (Becker, 1991). Numerous empirical and theoretical contributions have later challenged that specialization enhances union stability and increases fertility (Esping-Andersen and Billari, 2015; Goldscheider et al., 2015; Oppenheimer, 1997). In this section, we first explore how paternity leaves may affect the division of paid and unpaid work in the family, and then turn to the potential impact of paternity leave on union stability and fertility.

2.1 Paternity leave and gender specialization

If gender specialization is a cumulative or self-strengthening process, as suggested by Becker (1991), increasing fathers' participation in non-market work after the birth of a child can have a lasting impact on division of paid and unpaid work in the family. Increasing the length of fathers' parental leave may counteract the process of specialization in several ways, e.g. by enabling mothers to return to paid work sooner, by reducing her human capital depreciation, and/or by improving fathers' domestic skills. If an extended father's quota increases the father's skills in home production, the process of specialization may be then slowed down or even stopped. In support of this, Rehel (2014) finds that fathers acquired both new domestic skills and strengthened their emotional bonds with children after about one month of parental leave. Similarly, using a resource-bargaining perspective, Lundberg and Pollak (1996) suggest that strengthening of non-market skills among fathers and market skills among mothers lead to lasting, de-specializing impacts of the division of labor within families. Both the bonding between father and child and the acquisition of household skills can be stronger if fathers spend their leave without the presence of the mother, who in most cases holds a comparative advantage in house and childcare work.

Numerous studies have assessed the relationship between paternity leave and various family and child outcomes (see e.g. Schober 2014; Patnaik 2016; Østbakken et al. 2018 for reviews), and their findings primarily confirm that higher uptake of paternity leave is correlated with a more equal division of paid and domestic work. A small number of quasi-experimental studies address the de-specializing and earnings-equalizing potential of implementing father's quotas. Supporting the idea of de-specialization, some studies find positive effects on fathers' participation in child care (Cools et al., 2015; Schober, 2014) and house work (Kotsadam and Finseraas, 2011; Patnaik, 2016), and increases in mothers' labor supply (Kluve and Tamm, 2013; Patnaik, 2016). Meanwhile, other studies report negative effects on mothers' earnings (Cools et al., 2015) and labor supply (Ekberg et al., 2013), and increases in mothers' time spent on child care (Patnaik, 2016). Yet again, most studies find no causal effect on neither fathers' (Cools et al., 2015; Ekberg et al., 2013) nor mothers' (Rege and Solli, 2013) income, fathers' labor supply (Cools et al., 2015; Ekberg et al., 2013; Kluve and Tamm, 2013; Patnaik, 2016), or fathers' (Ekberg et al., 2013; Kluve and Tamm, 2013; Schober, 2014) or mothers' (Schober, 2014) participation in child care or house work.

Notably, for Norway Rege and Solli (2013) identify a substantial negative effect of the 1993 introduction of the father's quota on fathers' earnings using a difference-in-difference design – though only after a phase-in-period. In contrast, Cools et al. (2015) find no (negative) effects on father's earnings of the same reform. While Rege and Solli (2013) argue convincingly that one can only expect an effect when a larger proportion of fathers is moved by the reform, their identification strategy is also more vulnerable to bias from trends over time than that of Cools et al. (2015). Finally, Østbakken et al. (2018) analyze the effect of the Norwegian 2009 expansion of the paternity quota on a range of labor market related outcomes using a difference-in-difference design. They find that while the reform increased father's leave uptake, it did not permanently affect labor market outcomes. An exception is a small negative effect on mother's earnings in the short run, which they interpret as an increase in unpaid leave as a response to shorter paid parental leave.

2.2 Paternity leave and union stability

According to Amato (2007), conflict over unpaid work is among the major sources of marital dissatisfaction. Greenstein (2009) finds that a traditional division of unpaid labor is associated with lower relationship satisfaction for women in countries where men and women tend to share paid work. This pattern is confirmed in single country studies showing a negative association between traditional division of unpaid labor and women's relationship satisfaction (see e.g. Frisco and Williams (2003); Kaufman (2000) and Stevens et al. (2001) for the US; Kluwer et al. (1996) for the Netherlands; Barstad (2014) for Norway; Oláh and Gähler (2014) for Sweden). Men's efforts at home is also associated with lower union dissolution risk (Cooke (2006) for US; Sigle-Rushton (2010) for the UK; Ruppanner et al. (2017) for Sweden). Oláh and Gähler (2014) find that the combination of a gender equal ideology with a gender traditional practice lowers union stability among young Swedish coresidential couples. Similarly, Ruppanner et al. (2017) find that an unequal division of unpaid work is particularly detrimental to union stability if the extra work is put in by a woman, and not appreciated by her partner. These studies cannot be interpreted causally, as men who are satisfied with their union may be more inclined to do house and care work, and unmeasured characteristics such as personality traits may influence both men's housework and union stability. Still, they form the basis of the empirically testable prediction that his increased efforts at home will stabilize unions.

Mechanisms linking parental leave to increased union stability tend to depend on the parental leave invoking a lasting change in the division of household labor.[2] If the father increases his efforts at home, the mother's relationship satisfaction may increase, due to increased perceived fairness of the division of housework, and/or because house- and childcare may be more enjoyable as a shared than a solitary activity. The idea of perceived unfairness is rooted in equity theory (Adams 1965, see Lively et al. 2008 for applications to family research), which proposes that unfair social relationships give a feeling of distress, leading (particularly the discredited) actors to dissolve them. Of course, it is also possible that a more equal division of household work reduces relationship quality, to the extent that specialization fosters efficiency and this makes both partners more satisfied.

There are few previous studies that address the effect of paternity leave on union stability. Lappegård et al. 2019 find that a (somewhat) longer leave for fathers correlates with union stability in Norway, Sweden and Iceland (see also Oláh (2001) for a similar result for Sweden only). The authors acknowledge that this finding may fully or partly be driven by selection, i.e. more stable couples sharing leave more equally. Using a regression discontinuity/difference in difference design Cools et al. (2015), find no effect of the introduction of the paternity quota on marital dissolution after 14 years. Data limitations inhibit estimations of effects for the statistically less stable (Lyngstad and Jalovaara, 2010) cohabiting couples. In contrast, Avdic and Karimi (2018) find using an RD design that the introduction of a paternity quota in Sweden temporarily sped up union dissolutions among low-earning couples. In sum, these studies indicate that the positive relationship between paternity leave uptake and union stability may be due to selection. However, it is possible that extensions in contexts already favourable to paternal involvement have more profound effects on union stability.

2.3 Paternity leave and parity progression

To the extent that paternity leave reduces (increases) union stability, it should reduce (increase) fertility in the short run. However, one may also observe effects on fertility in lieu of effect on union dissolution. Following Goldscheider et al. (2015), we expect positive effects of father involvement to be mediated through two mechanisms. First, father involvement may increase relationship satisfaction in and of itself, as tasks may be more enjoyable when pursued jointly. Increased relationship satisfaction may again be positively associated with parity progression. Second, if his home production facilitates her market production, her opportunity costs of further childbearing will fall. If her opportunity costs blocked parity progression, fertility will then increase.

Increased father involvement may also lower fertility, as his increased opportunity cost may reduce his demand for children. The total effect depends on the size of the reduction in her opportunity costs relative to the increase in his (see Kravdal (2016) for a similar argument). Previous studies show that Norwegian men want fewer children than Norwegian women on average (Lyngstad and Noack, 2005), and that disagreement tends to lead to no further childbearing (Thomson, 1997). To the extent that men already tend to hold back family size, shifting opportunity costs from her to him may further reduce parity progression.

Previous analyses indicate a positive associtation betweeen paternity leave uptake and the risk of second births (Lappegård (2010) for Norway, Duvander et al. (2016a) for Norway, Sweden and Iceland). For higher order birth intensities, results are mixed, with a negative relationship found in Norway and Sweden and no relationship in Iceland (Duvander et al., 2016a; Lappegård, 2010). These studies highlight how selection into paternity leave might explain all or some of the observed differences in fertility outcomes. Causal studies of effects of paternity leave on fertility are rare. Cools et al. (2015) find no effects of the paternity quota on fertility after 14 years using a RD/DD design (see also Duvander et al. (2016b) for a somewhat less rigorous evaluation of the Norwegian and Swedish father quotas that yield largely the same results, but finding a temporary positive effect on fertility among Swedish low income couples). Farré and González (2017) utilize the implementation of a two-weeks father's quota in a regression discontinuity design in Spain and find that (short) paternity leave delay childbearing and reduce higher-order births among women aged 36 years or older. As for union dissolution, causal studies hint towards that the associations between paternity leave and fertility is largely driven by selection. However, again, there are indications of temporary ("tempo") reform effects.

2.4 Expectations

To form expectations of effects of paternity leave on family dynamics, we constrast two major perspectives on the relationship between gender equality and family wellbeing. Regarding union stability, the "gender revolution perspective" (Goldscheider et al., 2015) suggests that if paternity leave has a lasting impact on the father's efforts at home, it may increase the mother's relationship satisfaction and stabilize unions. A contrasting expectation, based on the standard microeconomic model (cf. Becker 1991), is that reduced specialization should reduce the gain from being in a union, and hence increase dissolution risk.

To the extent that paternity leave influences union stability, parity progression is likely affected in the same direction – at least in the short run. Furthermore, if paternity leave permanently reduces mother's "double burden", and this burden has suppressed parity progression, parity progression may pace up. However, a permanent increase in his opportunity cost of childbearing may also reduce his demand for children, and slow down parity progression. If his demand for children was already lower than hers, the latter mechanism may dominate, and the total effect will be negative.

As a proxy for changes of division of labor within the family, we estimate effects on various measures of market work. If the reform permanently strengthens his skills in home production, this may be reflected in weaker efforts in market work and a weaker earnings development.[3] If his and her efforts at home are substitutes, an increase in the mother's earnings will follow. Note that time may be shifted between (pure) leisure and unpaid work. Paid work can remain unmoved if he does more house- or care work, and she gets more (pure) leisure.

3 Reform details

The Norwegian parental leave system ensures income replacement and job security so employed parents can care for their new child. With an explicit goal of strengthening the relationship between father and child, as well as to improve the gender equality in the division of paid and domestic work between the parents (Norwegian Ministry for Children and the Family (1992) p. 30), the Norwegian government introduced a father's quota on April 1 1993. This policy reserved four weeks of leave exclusively[4] for the father, and divided the parental leave into a mother's quota, a father's quota and a period which could be divided freely between the parents. The duration of all three parts of the parental leave has gone through several changes since 1993, and the developments are summarized in Table 1. Parents can choose between 80 or 100 percent income replacement for a correspondingly longer or shorter leave. The social security system replaces earnings up to a cap of 6G[5], but several employers, including the Norwegian public sector, top up parental leave compensation for incomes above this cap.

As we can see from the table, the father's quota was expanded from the original four weeks to five weeks in 2005 and then to six weeks in 2006, with a corresponding one-week expansion in the total leave period in both these years. In 2009, however, the father's quota was expanded by four weeks, wherein only two weeks were added to the total leave period and the remaining two were shifted from the shared leave. This rather substantial policy change prompted a significant political debate, and was criticized for "taking" leave from the mother and "giving" it to the father – an argument that reflects the strong tendency for mothers to take all or most of the shareable leave (Dahl et al., 2014; Fougner, 2012).

All fathers whose child was born on or after the policy implementation date were eligible for the father's quota, as long as both parents had accumulated individual rights to paid parental leave. The eligibility criteria for paid parental leave have changed slightly over the period captured in the table, but for our sample (i.e. those who had a child close to July 1, 2009), eligibility depended on both parents having pensionable income for at least six out of the ten months before the child was born. Moreover, it was a requirement that the mother's eligibility was based on at least 50 percent employment (Norwegian Ministry for Children and the Family (2009): p.3).

There are requirements to the mother's labor market activity when the father uses the shared weeks in the paid parental leave, however, this is not the case when the father uses the father's quota (NAV, 2016). The mother could therefore, if desirable, stay at home together with the father on either paid holidays, unpaid leave or graded leave (Norwegian Ministry for Children and the Family (2009): p.3). However, Norwegian vacation legislation (entitling all employees in full time position to five weeks of paid vacation a year), combined with the rules on flexible leave uptake (NAV, 2015a) and the now 10 weeks father's quota, implied that it was impossible for the mother to stay at home during the entire father's quota without the family experiencing a drop in disposable income. It is therefore likely that the 2009 reform increased not only the number of leave days taken by fathers, but also the number of days that fathers spent alone with their child. In the qualitative analyses by Østbakken et al. (2018) many fathers highlighted that spending time alone with their child was important both for the development of domestic skills and for the opportunity to "bond" and build a strong and close relationship between father and child during the leave period (cf. Brandth and Kvande 2003; Brandt and Kvande 2018).

4 Methods and data

4.1 Identification strategy

The expansion of the father's quota was implemented July 1 2009, and our empirical strategy takes advantage of this clear cutoff in eligibility and the fact that families with children born just before and just after the cut-off should be very similar. We use the increase in the father's quota and the reduction in the shared leave as a discontinuous function of the birth date of the child to capture reform effects. Sharp RD takes the following basic form (Angrist and Pischke, 2014):

$$\mathrm{Y}_i=\alpha+\rho D_i+\gamma f(Z_i)+\varepsilon_i$$

Where α is a constant term, $\gamma f(Z_i)$ nets out trends in the assignment variable, D_i is a dummy variable for treatment, and ρ gives the reform effect on the outcome. The equation is estimated on both uptake and outcome variables using the Stata command rdrobust (Calonico et al., 2016), which specifies a local linear regression for $f(Z)$ using triangular kernel density estimation. Binwidth is estimated empirically, by an algorithm optimizing the tradeoff between less bias (narrower bins) and higher precision (wider bins). Binwidth is constrained to be identical on both sides of the cutoff. Our identifying assumption is that the specification of the running variable ($\gamma f(Z_i)$) nets out all variation correlated with the outcome and the running variable that is not due to the reform. For robustness, we also estimate simpler specifications (i.e. treatment dummy alone, and linear and quadratic specification of time) (Table A.5). We also show discontinuity plots with a linear fit at each side of the reform cutoff.

Selection around the cut-off may compromise identification (Tamm, 2013; Cools et al., 2015). Such self-selection into (or out of) eligibility could happen for two main reasons; by parents timing the conception of a child in anticipation of the reform, and by expectant parents with due dates close to July 1 postponing/speeding up induced births or planned caesarian sections. Families where the father is more involved in family matters will presumably time the birth to after the introduction, whereas families where the father is less involved might want to time the birth to before the introduction. These different types of families may differ in factors relevant for specialization and union stability too. Hence, if such strategic timing exists, comparing families with children born just before and just after the cut-off will yield biased results.

The intention to expand the father's quota to ten weeks was declared by the government in 2005 (Soria Moria 2005, p. 43), but the policy and its details (including date of implementation) was not proposed in the Council of State until April 3 2009 (Stortinget, 2015). This would leave less than nine months until the implementation, suggesting that strategic timing of conceptions should not be of major concern.[6] Cools et al. (2015) find strong evidence of strategic timing of births two weeks before and after the 1993 introduction of the fathers quota (c.f. Brenn and Ytterstad (1997)). Using placebo tests (testing for "effects" on earnings in the year prior to the reform) we do also find some evidence of strategic timing, with high-income couples shifting into the treatment group. When we exclude parents of children born the 13 days before and the 13 days after the reform, no such evidence remains. Hence, we keep this restriction in our main analyses.[7] We also present "donut plots" showing how RD estimates change when potential strategic timers are excluded from the sample (see Appendix Figures A.4 and A.3 for parental leave outcomes and sociodemographic outcomes respectively).

4.2 Data

Study samples

We base all analyses on data from Norwegian population registers covering the time period between 2007 and 2016. Our main study sample is women who gave birth to a child in May, June, July or August 2009. In Section 6, we test if restricting the sample to focal children born in June and July, or expanding it to focal children born March-October, yields similar results. Furthermore, it is required that the father and mother lived together as of January 1 2008 (before pregnancy). This restriction implies that treated parents on average have lived together for a longer time at the time of conception. We have tested if this influences the results by conditioning the sample on coresidence by January 1 2007, giving a more similar relationship duration requirement (on a relative scale) across treatment and control groups. Reassuringly, this condition yields similar results (available upon request).

We construct two samples for main analysis, one for sociodemographic outcomes and another for parental leave outcomes. For both samples we make three further restrictions. First, as we take interest in gender specialization, same-sex couples are excluded. Second, as an exogenous proxy for parental leave rights, we include only focal children whose mothers had earnings the year prior to the reform. Finally, as multiple births give rise to correlated observations, only one focal child per birth (and parental leave spell) is included in the sample. The final sociodemographic sample consists of 9 757 couples.

Measurement of parental leave outcomes requires one additional restriction, as leave spells are registered to parents rather than children. Hence, in order to link leave spells to focal children, we exclude couples who had another child 15 months before or after the focal child was born (see Appendix II for details), and then assume that any parental leave taken within 15 months is linked to the focal child. Note that this restriction may be problematic if it turns out that the reform influences fertility. Excluding children born 15 months after the focal child may imply that we exclude a higher share of parents if the reform indeed did have a positive effect on fertility. This will imply that the parental leave sample is endogenously conditioned. We will pay close attention to this in our estimations. The final parental leave sample consists of 9 516 couples. Descriptives for outcome variables are found in Appendix Table A.2.

For placebo analysis, we construct two samples mirroring the two main analysis samples (i.e. the socioldemographic sample and the parental leave sample), yet with all criterias shifted one year: The focal children are born in May, June, July or August 2008, parents must have co-resided as of January 1 2007, and the mother must be registered with positive earnings in 2007. Same-sex couples and children born in the 26 days around the placebo cutoff (July 1 2008) are excluded. The parental leave placebo sample consists of 9 110 couples, the sociodemographic placebo sample has 9 320 couples. Descriptive statistics for outcomes for both placebo samples are found in Appendix Table A.6.

4.2.1 Outcome variables

Measures of parental leave uptake As a first step, we establish whether our reform indeed has an effect on parental leave uptake among mothers and fathers. The main outcome of interest here is the number of paid leave days taken by the mother and father.[8] We also estimate effects on the number and average length of leave spells taken by the father and mother respectively, and each parent's propensity to take part time leave.[9] Together, these characteristics give an impression of whether the extended father's quota induced longer uninterrupted paternity leave spells. The effect on parents' leave uptake, if any, constitutes the mechanism or first stage through which effects on other outcomes are mediated. Descriptive statistics for all outcomes are shown in Table A.1. Details on the construction of parental leave data are given in Appendix II.

Union stability Our first demographic outcome of interest is union stability, measured yearly January 1st from 2011 (focal child one year old) to 2015 (focal child five years old). For each year, we construct a dummy variable taking the value one if the parental couple is still registered as living together, otherwise zero (see Appendix Table A.1 for descriptives). Unions are dissolved by registration of separate addresses. This register measure ensures zero attrition, crucial for the validity of our results. The death of one partner is a rare case of union dissolution among couples with young children, and unlikely to be influenced by parental leave uptake, and we hence consider it unlikely to bias our results. Descriptives for outcome variables are found in Appendix Table A.1.

Fertility Our second demographic outcome is subsequent fertility, that is, whether the focal child has younger sibling(s). We construct variables for the cumulative number of younger siblings born before the focal child's first (2010), second, third, fourth and fifth (2014) birthdays. Based on these count variables, we construct dummies for having at least one younger sibling within the same time frames.

Earnings Our starting point of the analyses of changes in market work is the sum of earned income and primary and secondary business income ("yrkesinntekt") (Steinkellner, 2003), an even better proxy of efforts in paid work than earned income alone. For brevity, we refer to this variable as earnings. Missing and zero earnings are set to 1, facilitating calculation of log earnings. We estimate effects both on the extensive margin (as captured by a dummy variable taking one if earnings exceed 1G, otherwise zero – see endnote 4) and the intensive margin (log earnings), for both mothers and fathers. Earnings are measured from 2010 (when focal child turns one years, and one parent is typically still on paid parental leave) to 2014 (when focal child turns five years). As paid parental leave is classified as earnings, parental leave with a subsequent child will not cause a drop in earnings. In addition to estimating effects on mothers' and fathers' earnings separately, we construct a measure for specialization in market work by dividing her earnings by the sum of her and his earnings.[10] An increase in this outcome means a shift towards a less traditional division of labor in the couple.

4.2.2 Control variables and subsample stratification

While a valid regression discontinuity design does not require inclusion of covariates beyond the running variable, covariates can both sharpen the precision of the estimates and provide robustness checks. Most importantly, we use information on observable characteristics measured prior to the reform (in 2008) to conduct subgroup analysis. Based on register information of marriages, we construct an indicator taking the value one if the parental union is a marriage, otherwise zero. We also construct a set of dummies for parity of the focal child, distinguishing between first borns, second borns, and later borns (merged to retain subsamples of meaningful size). We obtain information on educational attainment and enrollment from the National Educational Database (NUDB). When used as a control variable, educational attainment is grouped into four levels: Basic (not completed high school), completed high school, higher education lower degree (BA), and higher education higher degree (MA or PhD). To retain test strength, we collapse these categories into lower (basic and high school) and higher (higher and lower degree) for the subsample analysis. Missing information on education is coded as a separate fifth category. Individuals are defined as students if they have been enrolled in education for at least one month during the current year. We also conduct subsample analysis for younger couples (both under age 30 the year the focal child is born) and older couples (at least one parent aged 30 or above the year the focal child is born). When included as covariates, mother's and father's age are each included with a linear and curvilinear term.

5 Results

5.1 Reform effects on leave uptake

The reform incentivizes longer paid leave for fathers, and shorter paid leave for mothers. Effects on leave uptake are shown in Table 3, Panels A and B. No controls indicate the basic model with no covariates (beyond the running variable), whereas full controls imply estimates from a model where all covariates are included. For fathers (Panel A), the estimates show a substantial increase of about 14 leave days, both statistically significant and unaffected by inclusion of covariates. Keeping in mind that the reform increase the number of days reserved for the father from 30 to 50 days, and that fathers pre-reform on average took 33 paid leave days (Table 2, Panel A) this is a strong yet plausible increase. A visual RD (Figure 1a) confirms a clear jump in men's leave days at the cutoff. Furthermore, the percent of fathers who takes 10 weeks of paid leave or more, increases with 50 percentage points (Table 3 and Figure 1e, right panel), a massive increase from the pre-reform baseline of 12 percent (Table 2, Panel A). The mean duration of each of the father's parental leave spells is increased by 12 days, but there is no significant change in the number of spells taken. Neither fathers' propensity to take leave nor fathers' propensity to take part time leave are significantly affected.

The point estimates for mothers (Table 3, Panel B) show that the reform induced an average reduction in leave length of about 21 days, i.e. by about two weeks more than was incentivized by the reform. A visual RD for mother's number of leave days (Figure 1b) confirms a clear drop at the discontinuity. There is an equally large drop in the average duration of the leave spells for mothers (Figure 1d), suggesting that mothers still tend to use all of their leave in one continuous break from the labor market. Unsurprisingly, neither the mother's propensity to take leave nor the average number of parental leave spells are affected.

As fathers earn more than mothers in 3 of 4 couples (Table A.1), 80 per cent compensation implies a larger income loss (in absolute terms) for a large majority of couples when he takes a larger share of the leave. As such, the reform strengthens the incentive to choose 100 percent income compensation, and couples respond to this incentive by decreasing their propensity of taking 80 percent compensation (Table 3, Panel A). In other words, treated couples on average take fewer (yet better compensated) leave days. This shift to shorter total leave length might explain why mothers' number of leave days is reduced by more than the two weeks that were shifted to the father by the reform.

Subsample analysis (Appendix Table 4) show that effects are stronger for both fathers' and mothers' leave when the focal child is a boy, and statistically significant in this group only. [11] Effects on her leave are concentrated among mothers of first borns. Both for his and her leave length, effects are stronger for parents older than 22 years, and statistically significant in this group only. When tested in an interaction model, none of these group differences are significant at the 10 per cent level.

Taken together these findings show that the reform had a profound effect on the leave uptake of both mothers and fathers, confirming the findings from previous studies on the implementation of fathers' quotas (Cools et al., 2015; Ekberg et al., 2013; Geisler and Kreyenfeld, 2012; Patnaik, 2016). This substantial shift in the distribution of leave between parents means that the policy change is well suited to identify causal effects of increased paternal involvement during the first year after the child is born. As we observe changes in the leave uptake of both mothers and fathers, effects on other outcomes, if any, can be mediated by both fathers' increased time spent with a young child, and mothers' faster return to work after birth.

5.2 Effects of paternity leave on his and her market work

To further explore whether division of labor in the family was impacted by the reform, we estimate effects on his and her earned income. While we expect effects (if any) to be mediated by changes in division of parental leave, we present sharp RD (reduced form) estimates, whose validity does not hinge on changes in leave length being the only mechanism through which the reform affects other outcomes. His longer paternity leaves may reduce his earnings in the long run because he continues to pull more weight at home, and/or faces subsequent discrimination in the labor market. The combination of his longer and her shorter leave may strengthen her labor market outcomes.

Our main outcome of interest is (relative) earnings the year the focal child turns five (Table 3, Panel D). At this age, most Norwegian children are enrolled in a child care center, and a more permanent pattern of (absence of) specialization in the family is likely to have settled. A discontinuity plot (Figure 2c) shows no evidence of a jump in this variable at the reform cutoff. The point estimates of mother's share of earnings are positive, yet statistically insignificant. In other words, the tendency of mothers to provide about 40 percent of the household earnings remains unchanged throughout the period of observation. We also assess yearly effects up to the age of five (Figure 3g), starting in the year the focal child turns one, finding no significant effects in the preceding years. Our results hence provide convincing evidence that the extension of the fathers' quota did not change the mother's share of couple earnings.

We explore additionally whether there are effects on his or her earnings at both the intensive (log earnings) and the extensive margin (the probability of being employed, including on parental leave from employment) the year the focal child turns five (Table 3, Panel D). Starting with the effects on log earnings, point estimates are negative for both fathers and mothers, but never statistically significant from zero. Discontinuity plots show no visual evidence for a discontinuity for log earnings (Figures 2d and 2e). For effects on his and her propensity to be employed when the focal child turns five, estimates are small, insignificant and close to zero.

In the short run (the years the focal child turns one, two, three and four years) neither log earnings nor his propensity to be working are significantly affected for fathers (Figures 3c and 3e respectively). For mothers (Figure 3f), we find no effects on the propensity to be working in the short run. A statistically significant negative effect on log earnings emerges for mothers the year the focal child turns two, but disappears when the focal child is three and four years (Figure 3d). This may indicate that as some families respond to a shorter total paid leave with some unpaid leave or reduced working hours for the mother.[12]

We have also split the sample by union type, parents' age, mother's education, father's education and the sex and parity of the focal child, in order to explore whether these overall findings hide heterogeneous policy adaptions in different families (Table 5). There is a small tendency for the reform to reduce specialization in families where the mother has higher education (significant after controls only) ($p<0.1$). Interestingly, there is also a tendency of reduced specialization if the mother had at least two children before the focal child ($p<0.1$). In this group, only 10 per cent of mothers have another child within five years. This indicates that the reform may have lasting effects on despecialization that are masked by specialization due to parity progression when the focal child is first or second born (and 75 and 24 percent of mothers have another child within five years). However, none of these differences are significant at the 10 per cent level.

5.3 Effects on union stability

For union stability, our outcome of interest is whether the parental union is intact in a given year. As (relative) earnings was not moved by the reform, effects – if any – must run through mechanisms other than changed division of market work. Effects are estimated for the years the focal child is one (2010) through five (2015) years old. Positive estimates indicate a stabilizing effect. By construction of the sample, all unions are intact as of January 1 2008. Table A.1 shows that while 98 per cent of the parental unions remain intact when the focal child is one, the proportion gradually decreases to 90 per cent when the focal child is five.

Sharp RD estimates of the reform effect on the probability of the parental union to be intact when the focal child is five are found in Panel C of Table 3. The estimates are negative, but small and not statistically different from zero on the 95 per cent level. This is confirmed by the lack of a visible change in union stability around the cutoff, as can be observed in Figure 2a. In the short run (focal child aged one through four), point estimates are again small and statistically insignificant (Figure 3a).

Mean effects may mask heterogeneity, and we proceed to test for subgroup effects on union stability (when focal child is five years old) by splitting the by pre-reform characteristics (Table 6, upper panel). We find a negative effect on union stability among cohabiting couples, marginally significant ($p<0.1$) before controls, and significant ($p<0.05$) after. However, this estimate is strongly sensitive to exclusion around the cutoff, and the group differentials are not consistently significant at the 10 per cent level (results available upon request). Following Avdic and Karimi (2018), we also test for tempo effects in union dissolution separately by the mother's pre-reform earnings quintile (Figure A.2), again finding no effects.

5.4 Effects on parity progression

For parity progression, we investigate effects both on number of younger siblings, and on a dummy variable for having at least one younger sibling. Descriptive statistics show that 34 per cent of the sample has at least one younger sibling at age five (Table A.1). The average number of younger siblings at age 5 is 0.37, so that only a minority has more than one sibling. Number of younger siblings and the propensity to have a younger sibling is measured yearly at the focal child's birthday, ages one (2010) through five (2014).

Both for number younger sibling and the propensity of a younger sibling at age five, reform effects are negative yet statistically insignificant (see Table 3, lower panel). Supporting the absence of effects, we find no visual discontinuity at the cutoff in the probability of having a(nother) sibling within five years (2b). We note that relatively large standard errors means that there is a range of effects of meaningful size that we cannot reject. Through ages two to three years, we see a tendency of a negative effect, i.e. slower parity progression among the treated (Figure 3b). Yet, these estimates never reach statistical significance, and from the focal child is five years of age, point estimates are very close to zero.

Finally, we assess subsample effect on the probability of having at least one younger sibling at age five (Table 6, lower panel). The propensity to have an additional sibling varies strongly with parity, with 75 per cent of first borns, 24 per cent of second borns and 10 per cent of third or higher order having an additional sibling within five years. Despite these differences, we do not identify differential reform effects by parity. While there are some group differentials in point estimates, none of these differences are significant at the 10 per cent level.

6 Robustness checks

In addition to inclusion of exogenous covariates, we have conducted four robustness checks. We start by varying the inclusion criteria for our two study samples, first around the cutoff ("donut tests") (Section 6.1), and then narrowing and widening the birth months included (Section 6.2). We then test if the reform significantly affects pre-reform outcomes (Section 6.3). Finally, we perform a complete "placebo reform" (Section 6.4)

6.1 Donut tests: Varying exclusion around the cutoff

As discussed above, estimates may be biased because women strategically time delivery depending on their preferences for parental leave. To test whether such strategic timing influenes our results, we estimate "donut plots" showing point estimates for a total of 26 regressions per outcome, excluding one day at the time at each side of the cutoff. Ideally, the estimates we have presented above should be robust to further exclusion around the cutoff. As the sample size falls with increased exclusion (Figures A.3g and A.4f), so will precision, meaning that our focus is on comparing point estimates.

Figure A.3 shows donut tests for the parental leave variables. We see that characteristics of father's leave uptake, measured both in days (Figure A.3a), length of mean spell (Figure A.3c) and the proportion who takes at least 10 weeks of leave (Figure A.3e), are relatively stable across different exclusions around the cutoff. This supports that these estimates are not driven by random variation or timing. There is a tendency of larger point estimates for father's leave uptake without any exclusion around the cutoff, indicating that some couples may strategically time birth after the reform date due to a preference for a longer parental leave.

Estimates for mother's leave uptake are somewhat more sensitive to the window of exclusion. With no exclusion around the cutoff, estimates for mothers' total leave days (Figure A.3b) and mean length of spell (Figure A.3d) are positive and close to zero. When five days or more are excluded around the cutoff, estimates turn negative, yet they decrease in magnitude and loose significance when 15 or more days are excluded. Similarly, the effect on 80 per cent compensation (Figure A.3f) seems to depend strongly on the window of exclusion, and we can hence not rule out that this finding is due to a statistical fluke in our main sample.

Figure A.4 shows the results of donut tests for five sociodemographic outcomes measured when the focal child is five years old – union dissolution, having a younger sibling, and mother's and father's log earnings, as well as mother's share of family earnings. None of these outcomes were significantly affected by the reform in our main specification, and the donut plots show that this finding is robust across different exclusion rules.

In sum, our donut tests indicate that the effects on father's leave uptake are robust, while the magnitude of effects on her leave uptake and total leave length are more sensitive to specification. We note that effects on her leave length are consistently negative, so that the direction of the effect is robust. The null findings on sociodemographic outcomes are robust to varying exclusion around the cutoff.

6.2 Varying the width of the observation window

To further ascertain that trends in the running variable does not bias our results, we run our main models on both restricted samples (focal children born in June and July 2009) and extended samples (focal children born March to October 2009). We continue to exclude focal children born in the 13 days on each side of the cutoff, as in our main specification. Analysis of parental leave outcomes on the restricted sample (Appendix Table A.3) yields point estimates that are very similar to those found in the main sample for father's leave length (Table 3), yet the lower sample size reduces precision. For mother's leave length, estimates in the restricted sample are somewhat more negative than in the main sample.

When the inclusion window is extended (Appendix Table A.4), effects on father's leave length and uptake remain closely similar to those found in the main sample. For both total length of leave (80% compensation) and mother's leave length, estimates are attenuated when the window is extended. The gradual weakening of the effects for mothers with the extension of the sample window corroborates the impression of sensitivity from the donut test of these variables (Section 6.1). Yet, the finding of a negative effect on mother's leave length remains robust.

6.3 Effects on pre-reform outcomes

Our third robustness test pertains to estimating effects on pre-reform outcomes. As the parental leave sample is (potentially) endogenously conditioned, while the sociodemographic sample is not (see Section 4 for more details), tests are performed separately for the two samples. We show t-tests of mean differences (Table 7), discontinuity plots (Appendix Figures A.5 and A.6 for the parental leave and sociodemographic samples respectively) and regression discontinuity estimates (Table 8).

In the simple mean tests (Table 7), we test effects on a range of sociodemographic characteristics measured pre-reform (2008), including three market work outcomes (his and her log earnings, her share of couple earnings), parity of the focal child, measures of educational attainment and student status, the couple's propensity to be married. Two statistically significant differences emerge in both samples: Treated fathers earn on average less than untreated fathers, and treated mothers are 1 percentage point more likely to be enrolled in education than are untreated mothers.

In general, the discontinuity plots (Appendix Figures A.5 and A.6 for parental leave and sociodemographic samples respectively) show no clear visual discontinuites; the differences with respect to father's earnings and mother's student status are small yet discernible. When the running variable and exogenous controls are included in a RD model (Table 8), the effect on mother's student status is rendered insignificant, while the effect on fathers' earnings is significant at the ten per cent level only. No additional significant differences emerge in the RD pre-outcome models (Table 8). With the number of tests we perform, finding one significant difference at the ten per cent level is no more that one should expect from chance, and overall we consider the samples to be balanced. We note that the parental leave sample, that is in theory endogenously conditioned, does not fare worse in the balancing tests than does the sociodemographic sample.

6.4 Placebo reform

Our most important robustness test is the implementation of a "placebo reform" July 1 2008. The construction of the placebo reform sample mirror the construction of the main sample exactly, with all criteria and measurements shifted one year back (see Section 4.2). Descriptive statistics for the placebo test sample is shown in Appendix Table A.7. For parental leave outcomes (Panel A), we see that differences by placebo reform status are generally small and statistically insignificant. The exception is that "treated" couples are five precentage points less likely to take a longer leave, probably reflecting that children born later in the year tend to be offered a child care slot at a slightly younger age. For sociodemographic outcomes (Panel B), the mean differences indicate that the "treated" children are two percentage points less likely to have at least one younger sibling at age two, a difference that persists at age five.

We inspect discontinuity plots for the placebo reform (Appendix Figures A.7 and A.8 for parental leave and socioeconomic outcomes respectively), emphasising outcomes for which mean differences were found. For younger siblings (Appendix Figure A.8b), there is hardly any discernible visual discontinuity, while the plot for taking a overall shorter leave (Appendix Figure A.7f) reveals a clear discontinuity at the cutoff, although overlapping confidence intervals of binned data indicate that it may not be statistically significant. For other outcomes, there is little evidence of (potentially significant) discontinuities.

Turning to the RD estimates for the placebo analysis (Appendix Table A.8),the point estimates for father's leave days and father's propensity to take more than 50 days of leave are close to zero and statistically insignificant. Point estimates for mothers indicate that the "treated" mothers take 13-19 days less leave than the "untreated", yet this difference is rended insignificant after controls.[13] We identify a negative placebo effect ($p<0.1$) on the propensity to take part time leave, indicating some discontinous seasonality in this outcome only. Donut tests on parental leave estimates from the placebo sample (Appendix Figure A.9) confirm that the estimates from this sample are consistently statistically insignificant. Turning to effects on sociodemographic outcomes, placebo reform effects are small, zero to the first decimal or negative, and never statistically significant.

Balance tests on pre-reform variables (Table A.9) indicates that "treatment" and "control" are largely balanced before the running variable is netted out. In contrast to the main sample, "treatment" fathers now earn more than "control" fathers, yet the difference is smaller and no longer statistically significant. As in the main sample, there is significant imbalance on the proportion of student mothers, with 1 percentage point more students in the "treated" group. This indicates that the seasonality of fertility patterns may differ consistently for mothers who are enrolled in education and mothers who are not. As for the in the main sample, no differences are significant at the five per cent level when date and exogenous controls are included in a RD design (results available upon request).

6.5 Summary of robustness tests

In sum, the placebo test corroborates that effects on father's leave uptake, as well as the absence of effect of sociodemographic outcomes, are not an artefact of seasonality not captured by the running variable. This strengthens our interpretation that our main model correctly identifies effects on father's leave taking behavior, and that these changes do not translate into changes in the division of paid work, union stability and/or fertility. This impression is further corroborated by the same estimates being robust to variation in the sample window (both around the cutoff and at the outer margin). Simple t-tests show that the sample is largely balanced with respect to observable pre-reform characteristics, and imbalances do not exceed what one should expect from chance when the running variable is netted out in an RD. This further strengthens the causal interpretation of our results.

7 Concluding discussion

Increased father involvement has been suggested as a pathway to stabilize parental unions and increase fertility. Changes in policies intended to affect father involvement, such as paternity quotas in parental leave, may serve as a test of effects of father involvement on union stability and fertility. We add to the literature by analyzing an expansion of the paternity quota in 2009 in Norway. Before the introduction, the majority of fathers already took some parental leave, and likely due to an extensive supply of public child care slots from age one, long maternal career breaks upon the birth of a(nother) child had become rare (Rønsen and Kitterød, 2015). Extending paternity quotas in this context may have potential to affect patterns of work and care in families, and in turn shaping family dynamics. In contrast, at introduction, paternity qoutas tend to affect only a minority, and strong norms of care for small children as the mothers' work may prevail.

Our study utilizes the extension of the Norwegian paternity quota from 6 to 10 weeks as a source of exogenous variation in the length of paternity leave. We study reform effects on leave uptake, earnings, union stability and fertility in a Regression Discontinuity design, restricting our study sample to children born in the weeks around the implementation of the reform. The reform generated substantial and significant effects on leave uptake, inducing fathers who would have taken some leave regardless of the policy change to extend their leave with about 14 work days on average. The percent of fathers who takes at least 10 weeks of leave (the extended quota) increased with 50 percentage points from a baseline of 12 per cent. Mothers reduced their leave by about one month (21 work days) on average, but the exact length of this reduction is sensitive to specification. In terms of changing the division of labor in a child's first year of life, and providing an opportunity to strengthen the bond between father and child, the reform was a success. A battery of robustness checks supports that the variation in father involvement is truly exogenous, i.e. not driven by self-selection or seasonal variations in leave uptake.

Our results do not show effects on union dissolution when the focal child is five years old.

Hence, neither the expectation that increased father involvement would increase relationship satisifaction and stabilize unions (Goldscheider et al., 2015) nor that it would disrupt specialization and reduce union stability (Becker, 1991) were supported. We also test whether the reform shifts the timing of union dissolution by estimating effects when the focal child is aged one through four years, again finding no effects.

While union dissolution is not moved in a way we can measure, it is important to acknowledge that the reform may have caused incremental changes in relationship quality, not discernable with our data. To the best of our knowledge, no Norwegian data source combines the statistical strength required for an RD with self-reported data on relevant aspects of life in families – such as division of unpaid labor and relationship quality. A survey of such aspects, strategically administered to couples who had a child around the reform cutoff, would be invaluable to reform evaluation.

Parity progression is also unmoved by the reform, both in the medium term (five years), and in the short term (one through four years). Quicker parity progression may be an indicator of union quality, potentially easier to move than union stability itself. However, even if the reform did improve union quality, effects on parity progression may be depressed by the potentially larger opportunity cost of childbearing for fathers constituted by a longer paternity quota. There are no significant subsample differences when splitting by his and her education, parity, child sex, and parental age.

How do we interpret the lack of effects of paternity quotas on family dynamics? As a first step to understand mechanisms, we have estimated reform effects on division of paid work in the family, using this as a measure of changes in gender equality in the couples. Being exposed to a longer paternity quota may entail a learning effect, making fathers efficient in and aware of household chores, and leading them to take on more unpaid work also when the parental leave has come to an end. In short, we find no lasting effect on neither his or her earnings, neither on the intensive (log earnings) or extensive (probability of employment) margin. In the short run, we find a negative effect on her log earnings when the focal child turns two, presumably driven by an increase in unpaid leave as a response to a shorter total leave. Her share of couple earnings is consistently unmoved, in the short and long run. To the extent that changes in family dynamics were to be driven solely by changes in the division of paid work, it is unsurprising that the reform yielded no effects. Our results are in line by those obtained by Østbakken et al. (2018), published in a report with limited peer review, and with the effects of the introduction of the Norwegian paternity quota as analyzed by Cools et al. (2015). Our results indicate that even in a context where take-up is high, paternity quota does not move the earnings of women nor men. As such, our findings provide a contrast to those of Rege and Solli (2013). We note that division of unpaid labor may change in ways that leave paid work unaffected. In a Norwegian qualitative study, fathers who took long paternity leave report that their attachment with the child and their practical involvement were strengthened, though their work practices were mainly unaffected by this (Østbakken et al., 2018). Such changes could have implication for family stability and parity progression.

There is extensive scholarly debate on the link between father involvement on one side and fertility and union stability on the other. The dominant perspective that non-traditional families would give more union dissolutions and lower fertility (Becker, 1991) has been challenged by contributions suggesting the opposite effect (Cooke, 2006; Esping-Andersen and Billari, 2015; Goldscheider et al., 2015; Sigle-Rushton, 2010). The idea that father involvement strengthens families is intuitively appealing: it reconciles ideals of gender equality with ideals of stability of parous unions and relatively high fertility, indicating that *more*, not less, gender equality is the receipt for more children being raised in intact families. While far from a perfect test, our results cast some doubt on on the claim that father involvement – at least as induced by changes in parental leave policies – stabilizes unions and increases fertility. Of course, several aspects of father involvement are unmoved by this reform, and it is possible that gradual changes in fathering practices remain causally related to both union stability and subsequent childbearing. Still, as long as evidence for this hypothesis remains limited to estimates with potentially strong selection bias, one might also ponder if "more gender equality" simply is not as effective a pathway to more stable parental unions and higher fertility as hypothesized.

On a more positive note, our results are reassuring for policy makers who contemplate extensions of paternity quota, but are concerned that this may introduce or intensify fatherhood wage penalties. We find no evidence that fathers moved by the reform to extend their parental leave experienced such penalties. Of course, some fathers may have anticipated earnings penalties, and taken shorter or no leave despite the reform. Still, at least in the relatively family friendly

Norwegian environment, fathers who make use of extended paternity quotas to bond with their young children are neither putting their career prospects nor the family income at risk.

Notes

[1]The Nordic parental leave system offers parents a generous wage-compensation for staying home with a newborn child for around one year, and while the bulk of parental leave can be shared freely between the parents, it is in practice taken up mainly by the mother (Duvander and Lammi-Taskula, 2011; Lappegård, 2008).

[2]Because both mothers and fathers can shift time between paid/unpaid work and (pure) leisure, his increased effort at home may, but need not, be reflected in his lower earnings and/or her higher earnings.

[3]A negative effect on his earnings can also emerge from signaling; that is, that fathers who are induced by the reform to take longer leave faces subsequent discrimination ("wage penalties").

[4]The father's quota could not be transferred to the mother unless she was a single parent, the father was ineligible to paid parental leave, or the father was too sick or otherwise unable to care for the child.

[5]The base rate (G) of the Norwegian Social Insurance scheme is an annually adjusted amount used to define benefit eligibility and calculate pensions. As of 1. July 2009, the BA was 72 881 NOK, or 11 602 USD (calculated based on the exchange rated for 2009, `https://www.norges-bank.no/en/Statistics/exchange_rates/currency/USD`).

[6]It should be noted the public debate regarding the reform picked up in Norwegian newspapers as early as October 2008 (i.e. nine months prior to the implementation), but that it remains unlikely that future parents were able to guess the implementation date, as previous family policy reforms had been implemented on both April 1, May 1 and July 1.

[7]To avoid that the local polynomial regression adapts to the missing data around the cutoff, we add 13 to the running variable for all births before the cutoff, and subtract 13 to all births after the cutoff.

[8]Some parents are registered with a higher number of leave days than the parental leave system allows, potentially due to the erroneous registration of, e.g., sick leave days etc. during the paid parental leave period. Hence, we cap the leave duration at the maximum number of leave days available. The results are not sensitive to this.

[9]Tidskonto ("time account") allows parents to take leave days part time, for instance may the mother stay at home with the child certain days of the week, and the father stay at home the remaining days, see https://www.nav.no/fleksibeltuttak.

[10]Couples without earnings have equal earnings, and are assigned a value of 0.5.

[11]A donut test (available upon request) reveal that the very large negative point estimate for mother's leave length when the focal child is a boy is strongly sensitive to exclusion around the cutoff.

[12]Norwegian employees are entitled to a period of unpaid leave directly following parental leave to care for small children.

[13]To the extent that child care availability heaps around August in Norway, a shorter better paid leave in combination with holiday for each parent should be sufficient to cover care for a child born 1 week before August 1st (or later). 46 weeks of leave at 80 per cent compensation, minus 3 weeks mandatory for the mother before birth (assuming birth on due date), plus 10 weeks of paid holiday gives 53 weeks. Given our exclusion of four weeks around the cutoff, this corresponds closely to 100 percent compensation being more attractive to the treated in the main sample, and the "treated" in the placebo sample.

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Tables and Figures

Table 1: Development in the paid parental leave scheme, with 80/100 percent income coverage. Number of weeks.

Effective date	Reserved mother	Reserved father	To be shared	Total number of weeks
April 1 1992	9	-	24/33	33/42
April 1 1993	9	4	29/39	42/52
July 1 2005	9	5	29/39	43/53
July 1 2006	9	6	29/39	44/54
July 1 2009	9	10	27/37	46/56

Note: Of the weeks reserved for the mother, three weeks are to be used prior to giving birth, and an additional six immediately after. The father cannot take any of his leave days during this period. However, fathers may take 2 weeks of unpaid care leave during the first two weeks of the child's life. Several employers, including the Norwegian public sector, will allow the father to take paid leave these two weeks. This is unrelated to the father's quota.

Table 2: Mean differences by treatment status. Outcome variables. Parental leave sample (Panel A) and sociodemographic sample (Panel B)

Panel A: Parental leave outcomes			
	Post	Pre	Post - Pre
Father's days of leave (org.)	47.37	33.12	14.25***
Father's days of leave	47.05	33.09	13.95***
Father takes leave	0.78	0.77	0.01
Father takes >=10 weeks	0.63	0.12	0.51***
Father N leave spells	1.17	1.04	0.12***
Father mean duration spell	40.27	28.86	11.41***
Father uses time account	0.10	0.08	0.02**
Father 80% compensation	0.45	0.55	-0.10***
Mother's days of leave (org.)	208.62	227.86	-19.24***
Mother's days of leave	201.93	217.58	-15.64***
Mother takes leave	0.88	0.89	-0.01
Mother N leave spells	0.93	0.95	-0.01
Mother mean duration spell	202.87	221.31	-18.44***
Mother uses time account	0.02	0.01	0.00
Observations	9516		
Panel B: Sociodemographic outcomes			
	Post	Pre	Post - Pre
Mother's share 2y	0.39	0.39	-0.00
Mother's share 5y	0.40	0.39	0.01
Union intact 2y	0.98	0.98	0.00
Union intact 5y	0.89	0.90	-0.01
Father working 2y	0.95	0.96	-0.00
Father working 5y	0.95	0.95	-0.00
Father ln(earn) 2y	12.79	12.81	-0.02
Father ln(earn) 5y	12.84	12.87	-0.03
Mother working 2y	0.90	0.91	-0.01
Mother working 5y	0.91	0.91	-0.00
Mother ln(earn) 2y	12.02	12.10	-0.08
Mother ln(earn) 5y	12.20	12.21	-0.01
N younger sibs 2y	0.08	0.07	0.01
N younger sibs 5y	0.38	0.37	0.01
Has younger sib 2y	0.08	0.07	0.01
Has younger sib 5y	0.35	0.34	0.01
Observations	9757		

Note: The samples are opposite-sex couples with children born in 2009, either between May 1-June 17 (control) or July 14-August 31 (treatment). Couples must have co-resided as of January 1 2008, and the mother must be registered with earned income in 2008. In the parental leave sample, siblings (if any) must be born at least 16 months before/after the focal child.*** $p<0.001$, ** $p<0.01$, * $p<0.05$, † $p<0.1$.

Table 3: Reform effects on leave uptake and outcomes. Main samples. OLS/LPM estimates from Regression Discontinuity models.

	No controls			Controls		
	Est	(SE)		Est	(SE)	
A: LEAVE UPTAKE FATHERS						
Number of days	14,13	(3,58)	***	14,41	(3,69)	***
Use time account	0,00	(0,03)		0,00	(0,03)	
Takes leave	-0,02	(0,05)		-0,01	(0,04)	
Takes >= 50 days leave	0,49	(0,05)	***	0,50	(0,05)	***
Mean duration of spell	11,46	(3,29)	***	11,64	(3,40)	**
Number of spells	-0,09	(0,18)		-0,08	(0,17)	
80% compensation	-0,16	(0,06)	**	-0,15	(0,06)	**
B: LEAVE UPTAKE MOTHERS						
Number of days	-21,50	(8,61)	**	-20,82	(8,35)	**
Use time account	-0,02	(0,01)	†	-0,02	(0,01)	†
Takes leave	-0,02	(0,03)		-0,02	(0,03)	
Mean duration of spell	-23,38	(9,39)	**	-22,85	(9,22)	**
Number of spells	-0,03	(0,04)		-0,03	(0,04)	
C: DEMOGRAPHIC OUTCOMES						
Intact union ch. 5 y	-0,04	(0,03)		-0,03	(0,03)	
At least one younger sibling 5 y	-0,03	(0,05)		-0,04	(0,05)	
N younger siblings 5 y	-0,03	(0,06)		-0,04	(0,05)	
D: EARNINGS OUTCOMES						
Mothers' share ch. 5 y	0,03	(0,02)		0,03	(0,02)	
Father working ch. 5 y	-0,03	(0,03)		-0,03	(0,02)	
Father ln(earn.) ch. 5 y	-0,36	(0,30)		-0,30	(0,27)	
Mother working ch. 5 y	-0,01	(0,03)		0,00	(0,03)	
Mother ln(earn.) ch. 5 y	-0,10	(0,25)		-0,09	(0,29)	

Note: N=9 516 for the parental leave sample and 9 757 for the sociodemographic sample. The samples are opposite-sex couples with children born in 2009, either between May 1-June 17 (control) or July 14-August 31 (treatment). Couples must have co-resided as of January 1 2008, and the mother must be registered with earned income in 2008. In the parental leave sample, siblings (if any) must be born at least 16 months before/after the focal child. *** $p<0.001$, ** $p<0.01$, * $p<0.05$, † $p<0.1$.

Table 4: Reform effects on leave uptake. Subsample estimates. OLS/LPM estimates from Regression Discontinuity models.

	NO CONTROLS			FULL CONTROLS			
	EST.	(S.E.)		EST.	(S.E.)		N
FATHER'S LEAVE DAYS							
BY CHILD SEX							
Girl	10,29	(5,69)	†	6,92	(5,85)		4888
Boy	16,49	(4,98)	**	19,56	(4,80)	***	4628
BY (MOTHER'S) PARITY							
First born	14,40	(6,57)	*	14,06	(6,62)	†	2603
Second born	12,22	(5,22)	*	13,44	(5,33)	*	4499
Higher order	15,24	(6,74)	*	17,90	(6,29)	**	2414
BY UNION TYPE IN 2008							
Cohabiting	12,97	(5,12)	*	13,24	(5,00)	*	5014
Married	14,51	(5,61)	**	17,57	(5,60)	**	4502
BY FATHER'S EDUCATION							
No higher education	10,56	(4,66)	*	9,73	(4,92)	†	4871
Higher education	18,25	(4,76)	***	17,74	(4,59)	***	4483
BY MOTHER'S EDUCATION							
No higher education	10,54	(5,85)	†	10,39	(5,98)		3635
Higher education	17,61	(4,27)	***	17,93	(4,26)	***	5674
BY PARENT'S AGE							
Not young parents	14,32	(3,56)	**	15,03	(3,73)	***	1480
Young parents	12,89	(9,94)		14,41	(9,38)		8036
MOTHER'S LEAVE DAYS							
BY CHILD SEX							
Girl	-10,47	(13,11)		-10,67	(13,23)		4628
Boy	-44,61	(16,62)	**	-45,87	(16,62)	**	4888
BY (MOTHER'S) PARITY							
First born	-41,30	(20,56)	*	-40,28	(20,49)	*	2603
Second born	-14,80	(13,74)		-14,68	(13,55)		4499
Higher order	-24,93	(22,17)		-17,82	(19,53)		2414
BY UNION TYPE IN 2008							
Cohabiting	-22,89	(11,28)	*	-27,43	(11,84)	**	5014
Married	-19,02	(14,06)		-12,93	(13,62)		4502
BY FATHER'S EDUCATION							
No higher education	-29,79	(15,37)	*	-31,51	(15,81)	*	4871
Higher education	-13,47	(13,44)		-7,44	(13,41)		4483
BY MOTHER'S EDUCATION							
No higher education	-25,25	(18,62)		-27,39	(18,72)		3635
Higher education	-20,97	(10,44)	*	-19,09	(10,11)	*	5674
BY PARENT'S AGE							
Not young parents	-28,53	(9,65)	**	-26,18	(8,61)	***	1480
Young parents	12,50	(20,53)		9,29	(24,57)		8036

Note: For splits by union type and parity, subsamples sum to N=9 516. Splits by education sum to a lower N due to exclusion of individuals with missing educational attainment. The sample is opposite-sex couples with children born in 2009, either between May 1-June 17 (control) or July 14-August 31 (treatment). Couples must have co-resided as of January 1 2008, and the mother must be registered with earned income in 2008, and siblings (if any) must be born at least 16 months before/after the focal child. *** $p<0.001$, ** $p<0.01$, * $p<0.05$, † $p<0.1$.

Table 5: Reform effects on her share of earnings. Subsample estimates. OLS/LPM estimates from Regression Discontinuity models.

	NO CONTROLS			FULL CONTROLS			
	Est.	(S.E.)		Est.	(S.E.)		N
By child sex							
Girl	0,04	(0,03)		0,03	(0,03)		4747
Boy	0,01	(0,03)		0,02	(0,03)		5010
By (mother's) parity							
First born	-0,01	(0,03)		-0,01	(0,04)		2647
Second born	0,01	(0,03)		0,01	(0,03)		4629
Higher order	0,11	(0,05)	*	0,08	(0,05)	†	2481
By union type in 2008							
Cohabiting	0,02	(0,02)		0,02	(0,02)		5136
Married	0,03	(0,04)		0,03	(0,04)		4621
By father's education							
No higher education	0,03	(0,03)		0,03	(0,03)		5006
Higher education	0,02	(0,03)		0,02	(0,03)		4580
By mother's education							
No higher education	0,01	(0,04)		0,02	(0,04)		3755
Higher education	0,03	(0,03)		0,04	(0,02)	†	5788
By parent's age							
Not young parents	0,03	(0,02)		0,03	(0,02)	†	8227
Young parents	-0,01	(0,05)		0,01	(0,05)		1530

Note: For splits by union type, parity, child sex and parent's age, subsamples sum to N=9 757. Splits by education sum to a lower N due to exclusion of individuals with missing educational attainment. The sample is opposite-sex couples with children born in 2009, either between May 1-June 17 (control) or July 14-August 31 (treatment). Couples must have co-resided as of January 1 2008, and the mother must be registered with earned income in 2008. *** p<0.001, ** p<0.01, * p<0.05, † p<0.1.

Table 6: Reform effects demographic outcomes. Subsample estimates. OLS/LPM estimates from Regression Discontinuity models.

	NO CONTROLS			FULL CONTROLS			
	Est.	(S.E.)		Est.	(S.E.)		N
Probability of intact union							
By child sex							
Girl	-0,02	(0,05)		-0,03	(0,05)		4747
Boy	-0,06	(0,04)		-0,04	(0,04)		5010
By (mother's) parity							
First born	-0,07	(0,06)		-0,07	(0,06)		2647
Second born	-0,03	(0,05)		-0,02	(0,05)		4629
Higher order	-0,04	(0,06)		-0,03	(0,05)		2481
By union type in 2008							
Cohabiting	-0,07	(0,05)	†	-0,08	(0,05)	*	5136
Married	0,00	(0,04)		0,01	(0,04)		4621
By father's education							
No higher education	-0,03	(0,05)		-0,04	(0,05)		5006
Higher education	-0,04	(0,04)		-0,02	(0,04)		4580
By mother's education							
No higher education	-0,07	(0,06)		-0,07	(0,06)		3755
Higher education	-0,02	(0,04)		-0,02	(0,04)		5788
By parent's age							
Not young parents	-0,03	(0,04)		-0,02	(0,03)		8227
Young parents	-0,09	(0,08)		-0,09	(0,07)		1530
Subsequent sibling							
By child sex							
Girl	-0,01	(0,08)		-0,05	(0,07)		4747
Boy	-0,05	(0,07)		-0,04	(0,07)		5010
By (mother's) parity							
First born	-0,09	(0,09)		-0,08	(0,07)		2647
Second born	-0,04	(0,07)		-0,06	(0,07)		4629
Higher order	0,01	(0,07)		0,02	(0,07)		2481
By union type in 2008							
Cohabiting	-0,03	(0,06)		-0,06	(0,06)		5136
Married	-0,01	(0,08)		-0,03	(0,07)		4621
By father's education							
No higher education	-0,09	(0,07)		-0,08	(0,07)		5006
Higher education	0,06	(0,08)		0,00	(0,06)		4580
By mother's education							
No higher education	-0,01	(0,07)		-0,01	(0,07)		3755
Higher education	-0,03	(0,07)		-0,08	(0,06)		5788
By parent's age							
Not young parents	0,00	(0,05)		-0,02	(0,05)		8227
Young parents	-0,13	(0,14)		-0,10	(0,12)		1530

Note: For splits by union type, parity, child sex and parent's age, subsamples sum to N=9 757. Splits by education sum to a lower N due to exclusion of individuals with missing educational attainment. The sample is opposite-sex couples with children born in 2009, either between May 1-June 17 (control) or July 14-August 31 (treatment). Couples must have co-resided as of January 1 2008, and the mother must be registered with earned income in 2008. *** $p<0.001$, ** $p<0.01$, * $p<0.05$, † $p<0.1$.

Table 7: Mean differences by treatment status. Balancing tests on pre-reform characteristics. Parental leave sample (Panel A) and sociodemographic sample (Panel B)

Panel A: Parental leave sample	Post	Pre	Post - pre
Married	0.48	0.47	0.00
Parity	2.05	2.05	0.00
Mother's age	31.52	31.55	-0.03
Mother's earnings	311570.39	313522.27	-1951.87
Mother's share earnings	0.41	0.41	0.01
Mother basic educ.	0.12	0.11	0.00
Mother HS educ.	0.27	0.28	-0.01
Mother higher educ., lower degr.	0.44	0.44	0.00
Mother higher educ., higher degr.	0.15	0.14	0.01
Mother missing educ.	0.02	0.02	-0.00
Mother student	0.10	0.09	0.01*
Father's age	34.35	34.23	0.12
Father's earnings	472392.17	489399.85	-17007.69*
Father basic educ.	0.17	0.15	0.01
Father HS educ.	0.39	0.41	-0.01
Father higher educ., lower degr.	0.28	0.27	0.01
Father higher educ., higher degr.	0.14	0.16	-0.01
Father missing educ.	0.02	0.02	0.00
Father student	0.07	0.06	0.01
Observations	9516		
Panel B: Sociodemographic sample	**Post**	**Pre**	**Post - pre**
Married	0.48	0.47	0.01
Parity	2.05	2.05	-0.00
Mother's share earnings	0.41	0.40	0.01
Mother's age	31.48	31.53	-0.05
Mother employed	0.95	0.95	-0.00
Mother's earnings	310798.04	312530.61	-1732.57
Mother basic educ.	0.12	0.11	0.00
Mother HS educ.	0.27	0.29	-0.01
Mother higher educ., lower degr.	0.44	0.44	-0.00
Mother higher educ., higher degr.	0.14	0.14	0.01
Mother missing educ.	0.02	0.02	0.00
Mother student	0.10	0.09	0.01*
Father's age	34.33	34.21	0.11
Father employed	0.96	0.97	-0.01**
Father's earnings	472245.40	488204.97	-15959.57*
Father basic educ.	0.17	0.16	0.01
Father HS educ.	0.39	0.41	-0.01
Father higher educ., lower degr.	0.28	0.27	0.01
Father higher educ., higher degr.	0.14	0.16	-0.01
Father missing educ.	0.02	0.02	0.00
Father student	0.07	0.06	0.01
Observations	9757		

Note: The samples are opposite-sex couples with children born in 2009, either between May 1-June 17 (control) or July 14-August 31 (treatment). Couples must have co-resided as of January 1 2008, and the mother must be registered with earned income in 2008. In the parental leave sample, siblings (if any) must be born at least 16 months before/after the focal child.*** $p<0.001$, ** $p<0.01$, * $p<0.05$, † $p<0.1$.

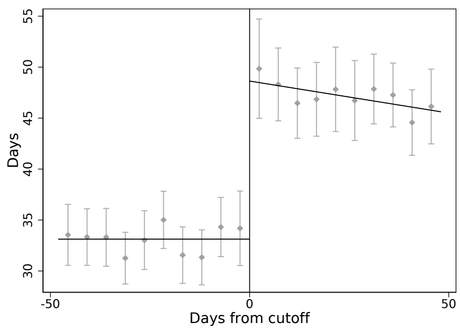
Table 8: Robustness check: Reform effects on pre-reform outcomes. Main samples. OLS/LPM estimates from Regression Discontinuity models.

	No controls			Controls		
	Est	(SE)		Est	(SE)	
Panel A: Parental leave sample						
Married	-0,03	(0,05)		-0,03	(0,05)	
Parity	-0,09	(0,11)		-0,07	(0,11)	
Mother						
Share couple earnings	-0,01	(0,02)		0,00	(0,02)	
Earnings	-16062,52	(18027,72)		-7470,06	(16648,11)	
Is working	0,00	(0,02)		0,00	(0,02)	
Basic educ.	0,00	(0,04)		-0,01	(0,04)	
HS educ.	0,01	(0,05)		0,00	(0,04)	
Higher educ., lower	0,00	(0,05)		0,00	(0,05)	
Higher educ., upper	-0,02	(0,04)		-0,01	(0,04)	
Missing educ.	0,02	(0,02)		0,02	(0,02)	
Enrolled in educ	-0,02	(0,03)		-0,02	(0,03)	
Father						
Earnings	-38985,45	(25930,93)		-42617,76	(25407,58)	†
Is working	-0,02	(0,02)		-0,02	(0,02)	
Basic educ.	-0,01	(0,04)		-0,02	(0,04)	
HS educ.	0,04	(0,05)		0,04	(0,05)	
Higher educ., lower	-0,02	(0,05)		-0,02	(0,05)	
Higher educ., upper	-0,01	(0,03)		-0,01	(0,03)	
Missing educ.	0,01	(0,01)		0,01	(0,01)	
Enrolled in educ	-0,03	(0,03)		-0,02	(0,03)	
Panel B: Socioeconomic sample						
Married	-0,02	(0,05)		-0,02	(0,05)	
Parity	-0,10	(0,11)		-0,07	(0,10)	
Mother						
Share couple earnings	-0,01	(0,02)		0,00	(0,02)	
Earnings	-17425,41	(18387,09)		-8524,97	(16934,34)	
Is working	-0,01	(0,02)		0,00	(0,02)	
Basic educ.	0,00	(0,04)		0,00	(0,04)	
HS educ.	0,00	(0,05)		-0,01	(0,04)	
Higher educ., lower	0,00	(0,05)		0,00	(0,05)	
Higher educ., upper	-0,02	(0,04)		0,00	(0,03)	
Missing educ.	0,02	(0,02)		0,01	(0,02)	
Enrolled in educ	-0,02	(0,03)		-0,02	(0,03)	
Father						
Earnings	-41350,39	(25702,54)	†	-44909,28	(24972,80)	†
Is working	-0,02	(0,02)		-0,02	(0,02)	
Basic educ.	-0,01	(0,04)		-0,01	(0,04)	
HS educ.	0,04	(0,05)		0,03	(0,05)	
Higher educ., lower	-0,02	(0,04)		-0,01	(0,05)	
Higher educ., upper	-0,01	(0,03)		-0,01	(0,03)	
Missing educ.	0,01	(0,01)		0,01	(0,01)	
Enrolled in educ	-0,02	(0,03)		-0,02	(0,03)	

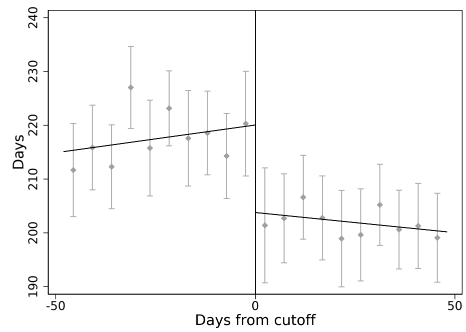
Note: N=9 516 for the parental leave sample and 9 757 for the sociodemographic sample. The samples are opposite-sex couples with children born in 2009, either between May 1-June 17 (control) or July 14-August 31 (treatment). Couples must have co-resided as of January 1 2008, and the mother must be registered with earned income in 2008. In the parental leave sample, siblings (if any) must be born at least 16 months before/after the focal child. *** $p<0.001$, ** $p<0.01$, * $p<0.05$, † $p<0.1$

Figure 1: Reform effects on leave uptake measures. Discontinuity plots. Lines give linear fit on each side of the cutoff. Points give bin-specific means, error bars give their 95 per cent confidence intervals.

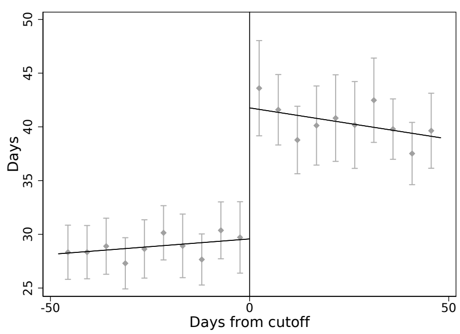
(a) Fathers leave uptake in days.



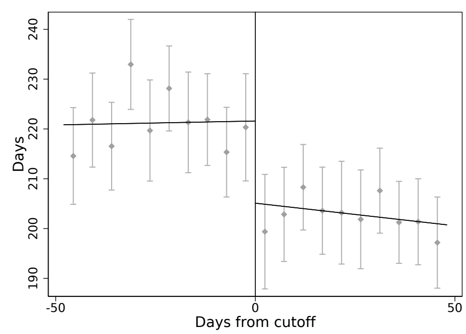
(b) Mother's leave uptake in days



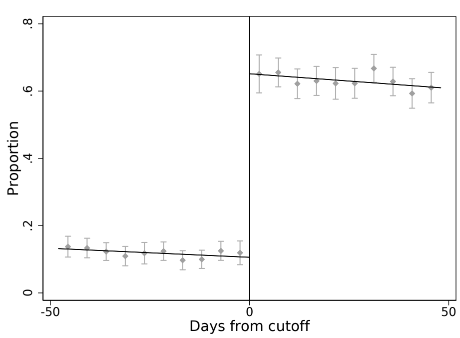
(c) Mean leangth of spell, fathers.



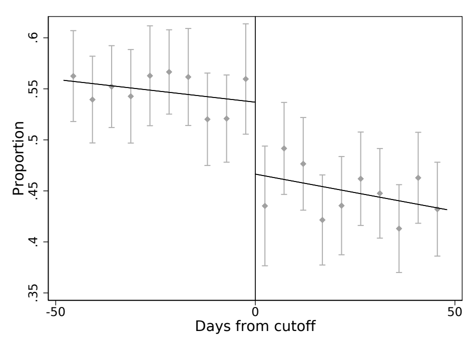
(d) Mean length of spell, mothers.



(e) Father's probability of taking at least 50 work days of leave.



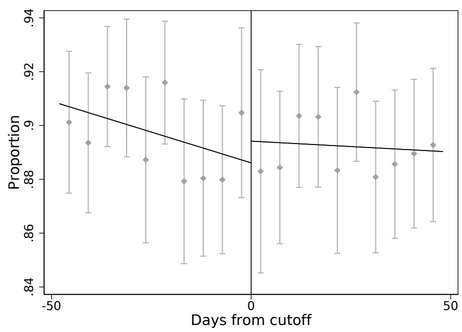
(f) Longer, less compensated leave.



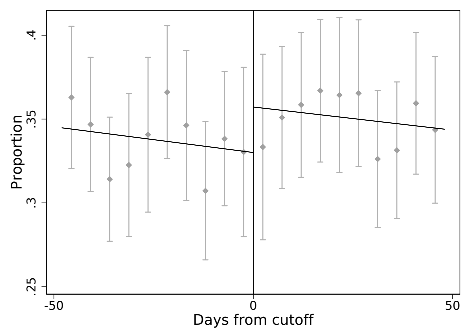
Note: N=9 516. The sample is opposite-sex couples with children born in 2009, either between May 1-June 17 (control) or July 14-August 31 (treatment). Couples must have co-resided as of January 1 2008, and the mother must be registered with earned income in 2008, and siblings (if any) must be born at least 16 months before/after the focal child.

Figure 2: Reform effects on sociodemographic outcomes. Discontinuity plots. Lines give linear fit on each side of the cutoff. Points give bin-specific means, error bars give their 95 per cent confidence intervals. .

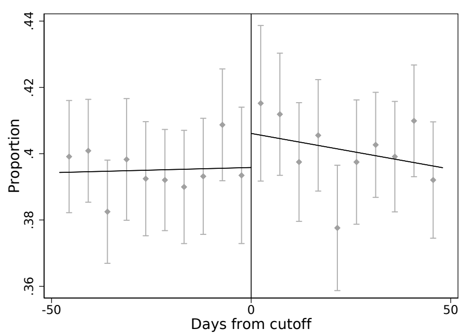
(a) Union intact after 5 years.



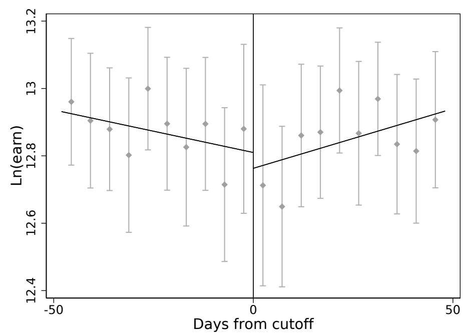
(b) Younger sibling within 5 years



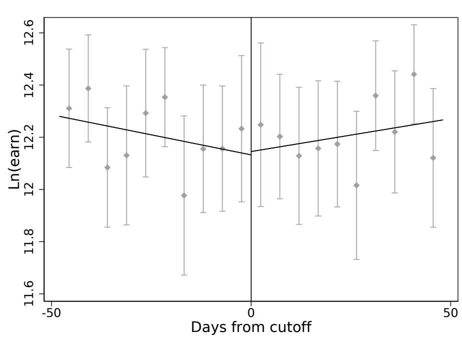
(c) Her share of earnings after 5 years.



(d) Father's log earnings after 5 years.



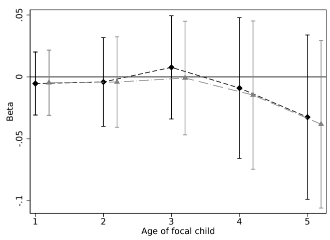
(e) Mother's log earnings after 5 years.



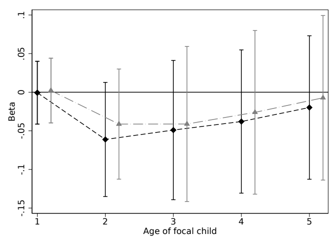
Note: N=9 757. The sample is opposite-sex couples with children born in 2009, either between May 1-June 17 (control) or July 14-August 31 (treatment). Couples must have co-resided as of January 1 2008, and the mother must be registered with earned income in 2008.

Figure 3: Reform effects on sociodemographic outcomes. OLS/LPM estimates from Regression Discontinuity models, estimated separately by the age of the focal child. Dots mark point estimates and error bars 95 per cent confidence intervals. Black lines indicates basic model, grey full controls.

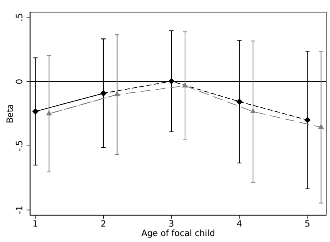
(a) Union intact.



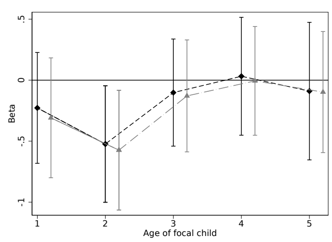
(b) Additonal sibling



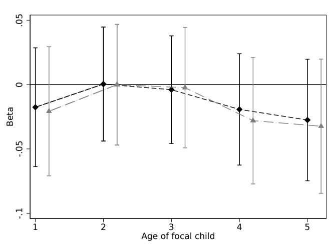
(c) Father's log earnings.



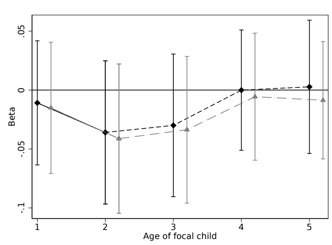
(d) Mother's log earnings.



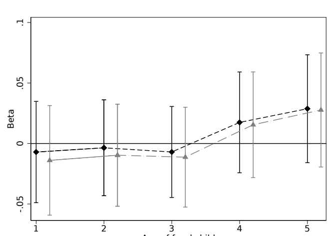
(e) Father's employment.



(f) Mother's employment.



(g) Her share of earnings.

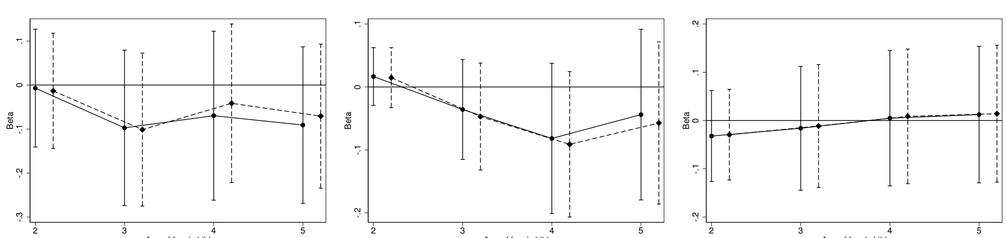


Note: N=9 757. The sample is opposite-sex couples with children born in 2009, either between May 1-June 17 (control) or July 14-August 31 (treatment). Couples must have co-resided as of January 1 2008, and the mother must be registered with earned income in 2008.

Appendix I: Additional tables and figures

Figure A.1: Timing effects by subgroup on parity progression. Dots give OLS/LPM estimates from Regression Discontinuity models and error bars their 95 per cent confidence intervals.

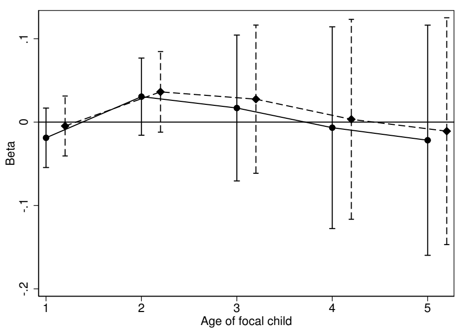
(a) First borns (b) Second borns (c) Third and higher order



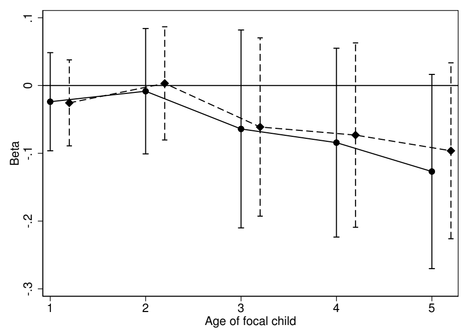
Note: N=9 757. The sample is opposite-sex couples with children born in 2009, either between May 1-June 17 (control) or July 14-August 31 (treatment). Couples must have co-resided as of January 1 2008, and the mother must be registered with earned income in 2008.

Figure A.2: Timing effects by subgroup on union dissolution. Dots give OLS/LPM estimates from Regression Discontinuity models and error bars their 95 per cent confidence intervals.

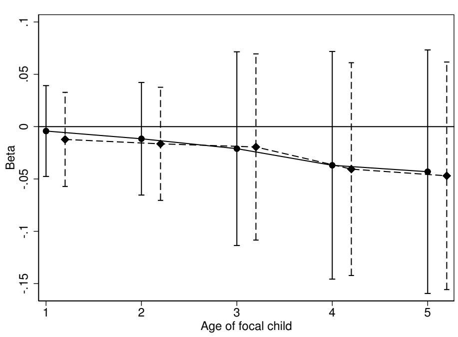
(a) Her earnings Q1



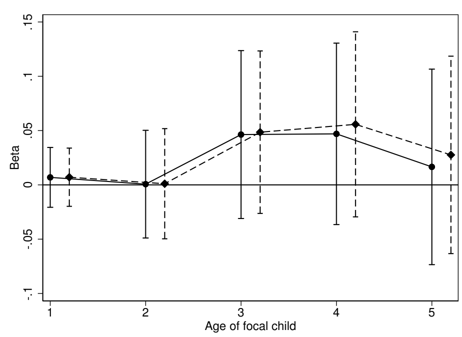
(b) Her earnings Q2



(c) Her earnings Q3



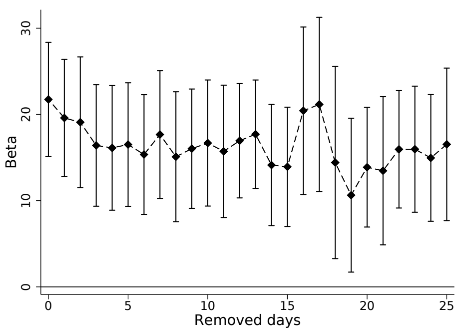
(d) Her earnings Q4



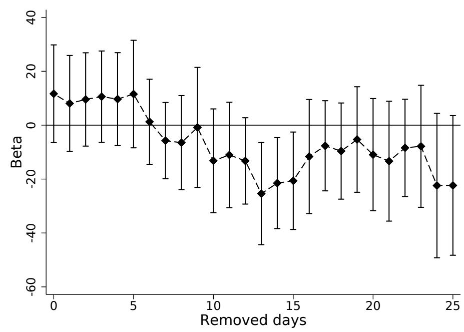
Note: N=9 757. The sample is opposite-sex couples with children born in 2009, either between May 1-June 17 (control) or July 14-August 31 (treatment). Couples must have co-resided as of January 1 2008, and the mother must be registered with earned income in 2008.

Figure A.3: Donut plots for uptake, main sample. Dots give OLS/LPM estimates from Regression Discontinuity models and error bars their 95 per cent confidence intervals.

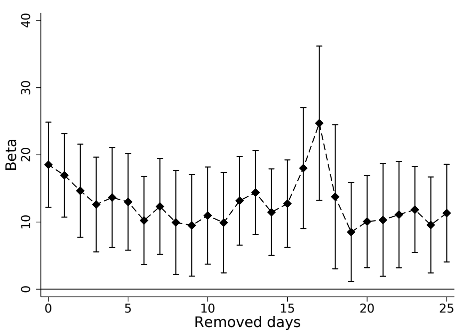
(a) Fathers leave uptake in days.



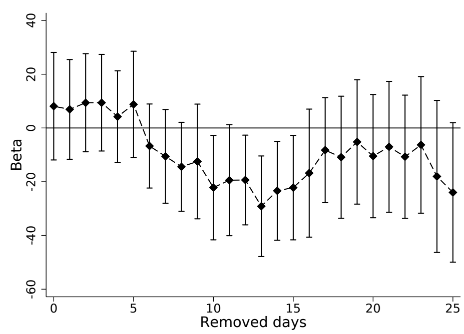
(b) Mother's leave uptake in days



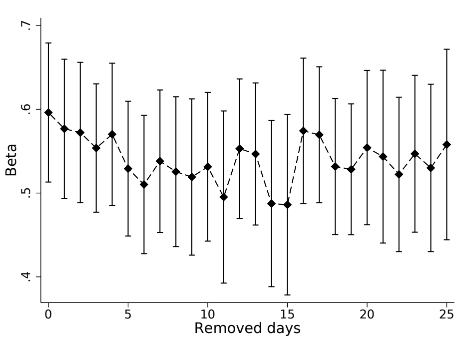
(c) Mean leangth of spell, fathers.



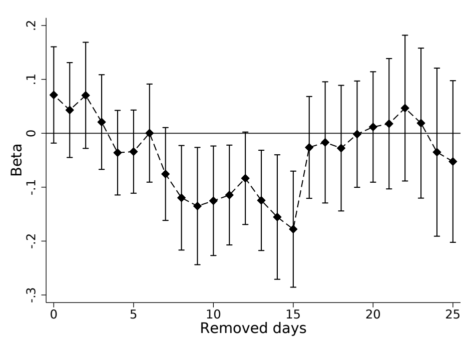
(d) Mean length of spell, mothers.



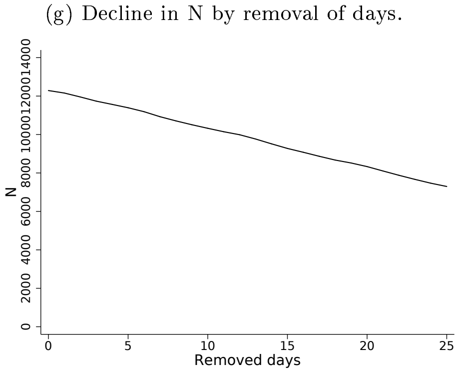
(e) Father's probability of taking at least 50 work days of leave.



(f) Leave with 80 per cent compensation.



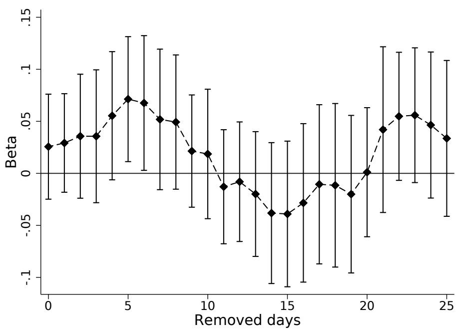
(g) Decline in N by removal of days.



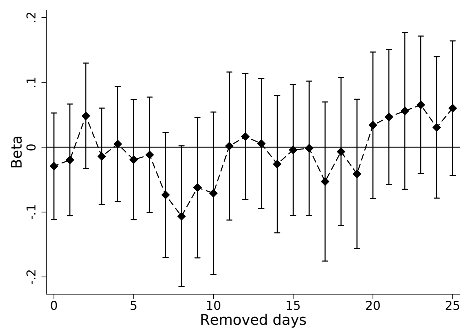
Note: The sample is opposite-sex couples with children born in 2009, either between May 1-June 17 (control) or July 14-August 31 (treatment). Couples must have co-resided as of January 1 2008, and the mother must be registered with earned income in 2008, and siblings (if any) must be born at least 16 months before/after the focal child.

Figure A.4: Donut plots for sociodemographic outcomes, main sample. Dots give OLS/LPM estimates from Regression Discontinuity models and error bars their 95 per cent confidence intervals.

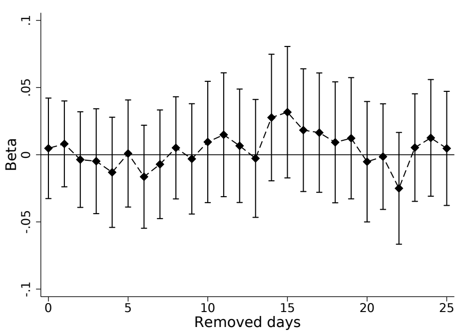
(a) Union intact after 5 years.



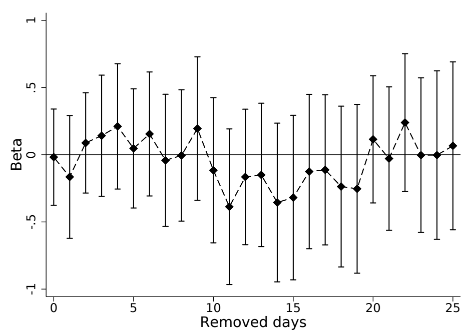
(b) Younger sibling within 5 years



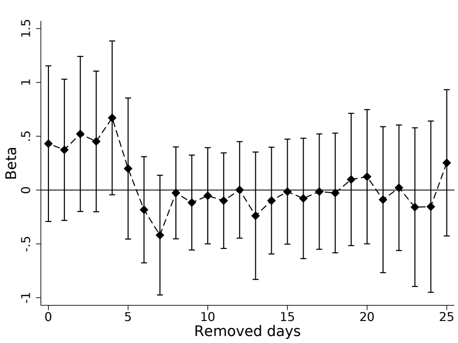
(c) Her share of earnings after 5 years.



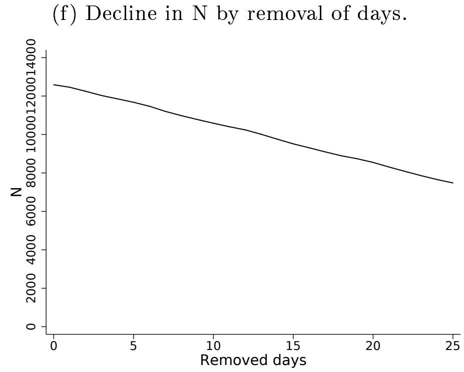
(d) Father's log earnings after 5 years.



(e) Mother's log earnings after 5 years.



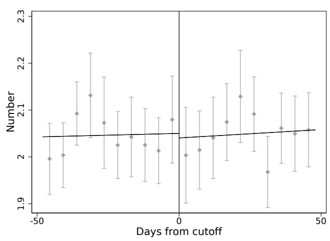
(f) Decline in N by removal of days.



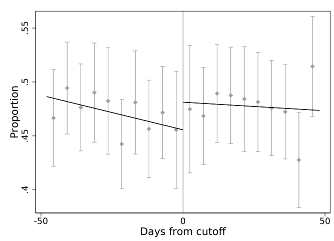
Note: The sample is opposite-sex couples with children born in 2009, either between May 1-June 17 (control) or July 14-August 31 (treatment). Couples must have co-resided as of January 1 2008, and the mother must be registered with earned income in 2008.

Figure A.5: Effects on pre-reform outcomes, parental leave sample. Discontinuity plots. Points give bin-specific means, error bars give their 95 per cent confidence intervals. Lines give linear fit..

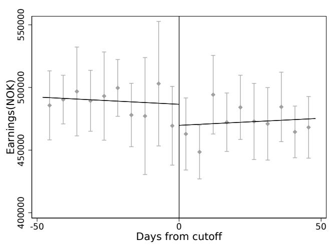
(a) Parity of focal child



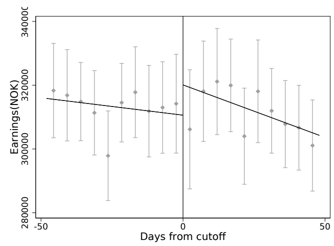
(b) Married before birth of focal child



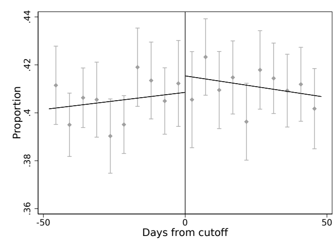
(c) Father's earnings



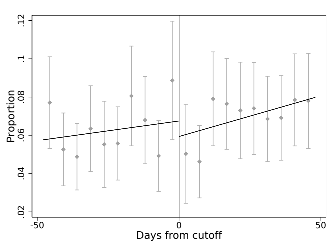
(d) Mother's earnings



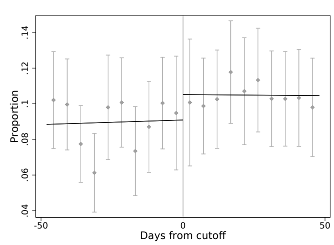
(e) Mother's share of couple earnings



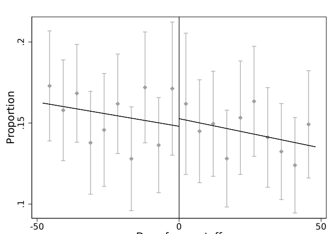
(f) Father is student



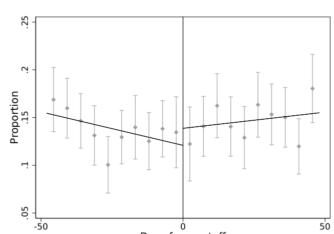
(g) Mother is student



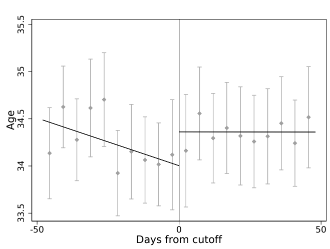
(h) Father has higher education



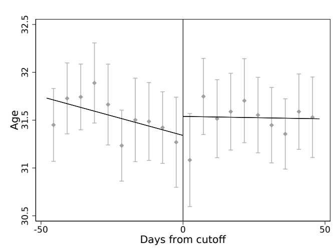
(i) Mother has higher education



(j) Father's age



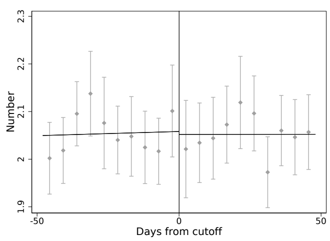
(k) Mother's age



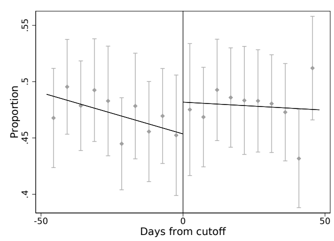
N=9 516. The sample is opposite-sex couples with children born in 2009, either between May 1-June 17 (control) or July 14-August 31 (treatment). Couples must have co-resided as of January 1 2008, and the mother must be registered with earned income in 2008, and siblings (if any) must be born at least 16 months before/after the focal child.

Figure A.6: Effects on pre-reform outcomes, sociodemographic sample. Discontinuity plots. Points give bin-specific means, error bars give their 95 per cent confidence intervals. Lines give linear fit.

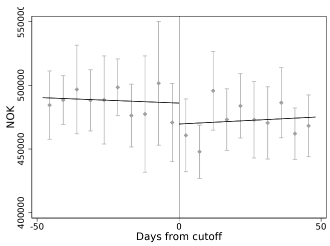
(a) Parity of focal child



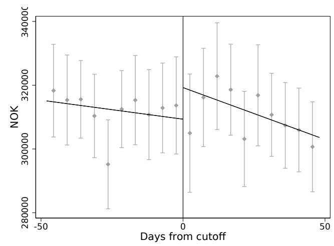
(b) Married before birth of focal child



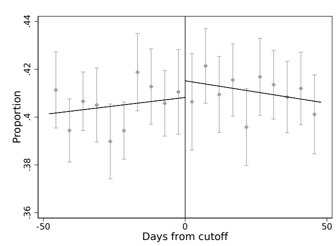
(c) Father's earnings



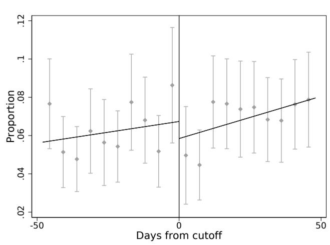
(d) Mother's earnings



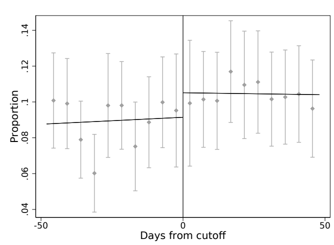
(e) Mother's share of couple earnings



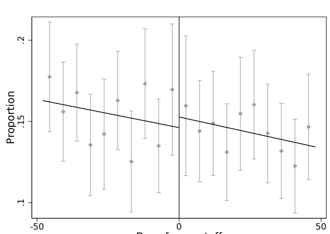
(f) Father is student



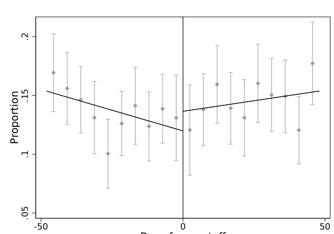
(g) Mother is student



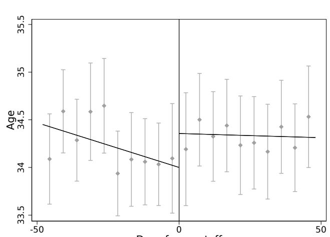
(h) Father has higher education



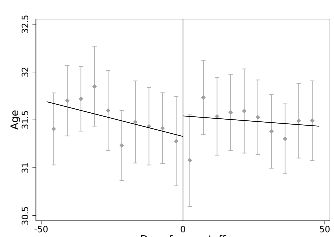
(i) Mother has higher education



(j) Father's age



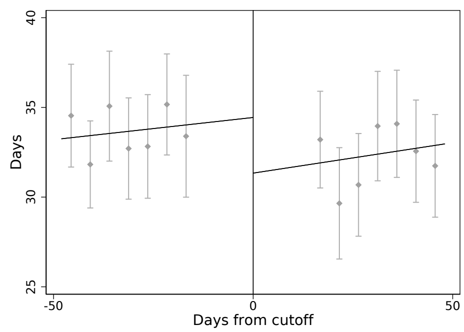
(k) Mother's age



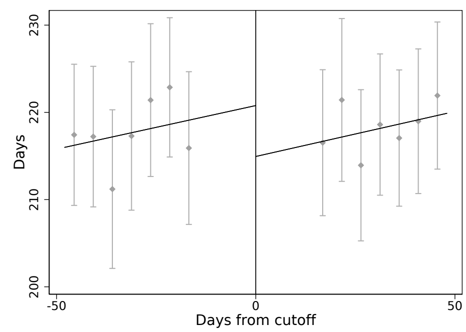
Note: N=9 757. The sample is opposite-sex couples with children born in 2009, either between May 1-June 17 (control) or July 14-August 31 (treatment). Couples must have co-resided as of January 1 2008, and the mother must be registered with earned income in 2008.

Figure A.7: Reform effects on leave uptake measures, placebo sample. Discontinuity plots. Points give bin-specific means, error bars give their 95 per cent confidence intervals. Lines give linear fit.

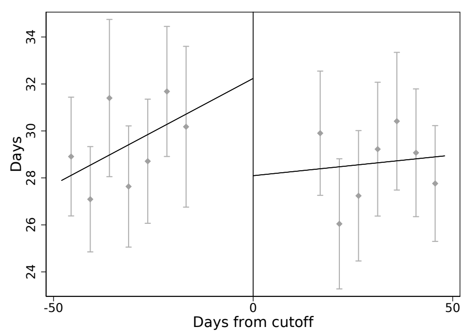
(a) Fathers leave uptake in days.



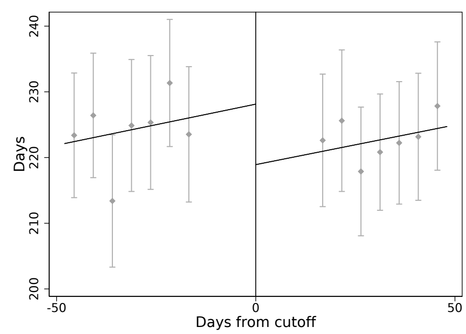
(b) Mother's leave uptake in days



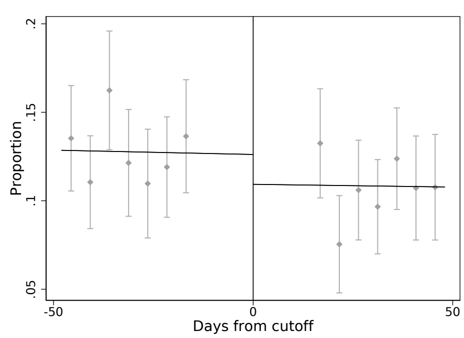
(c) Mean leangth of spell, fathers.



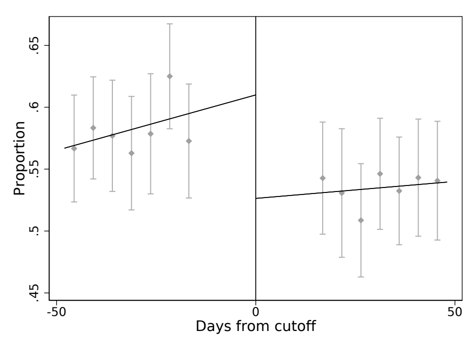
(d) Mean length of spell, mothers.



(e) Father's probability of taking at least 50 work days of leave.



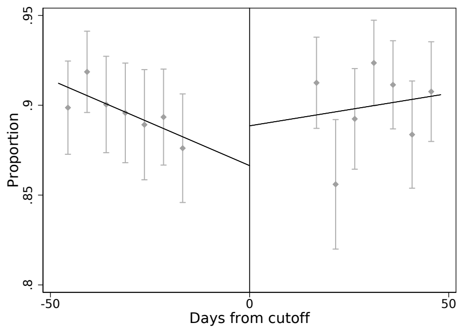
(f) Probability of longer less compensated leave.



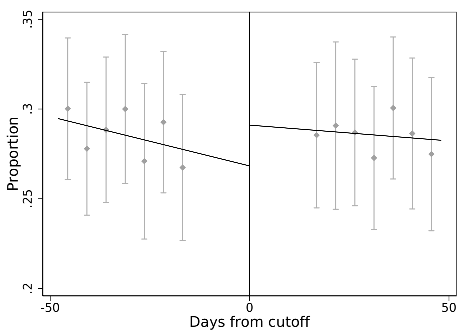
Note: N=9 110. The sample is opposite-sex couples with children born in 2008, either between May 1-June 17 (control) or July 14-August 31 (treatment). Couples must have co-resided as of January 1 2007, and the mother must be registered with earned income in 2007, and siblings (if any) must be born at least 16 months before/after the focal child.

Figure A.8: Reform effects on sociodemographic outcomes, placebo sample. Discontinuity plots. Points give bin-specific means, error bars give their 95 per cent confidence intervals. Lines give linear fit.

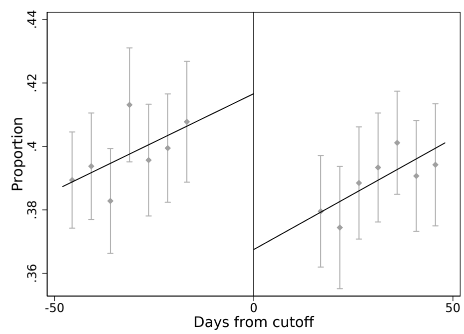
(a) Union intact after 5 years.



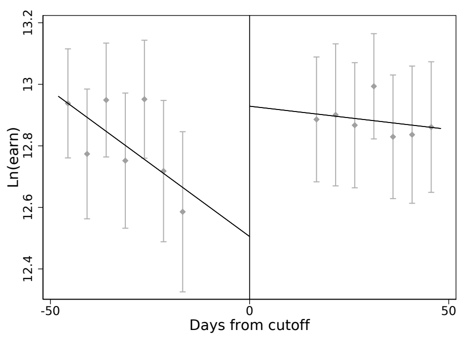
(b) Younger sibling within 5 years



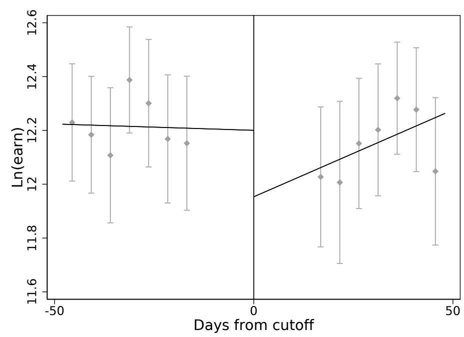
(c) Her share of earnings after 5 years.



(d) Father's log earnings after 5 years.



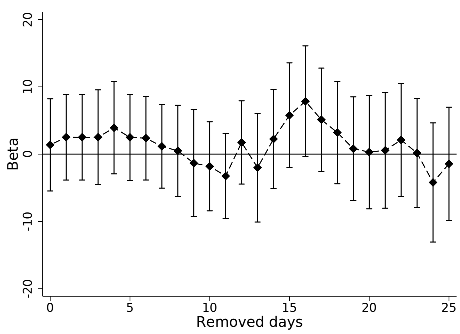
(e) Mother's log earnings after 5 years.



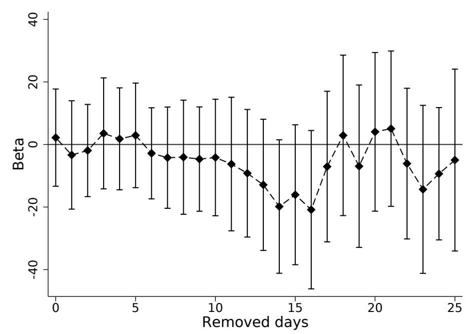
Note: N=9 320.The sample is opposite-sex couples with children born in 2008, either between May 1-June 17 (control) or July 14-August 31 (treatment). Couples must have co-resided as of January 1 2007, and the mother must be registered with earned income in 2007.

Figure A.9: Donut plots for uptake, placebo sample. Dots give OLS/LPM estimates from Regression Discontinuity models and error bars their 95 per cent confidence intervals.

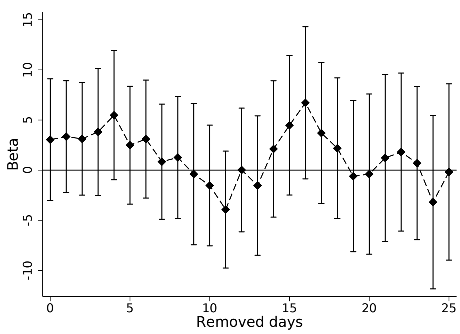
(a) Fathers leave uptake in days.



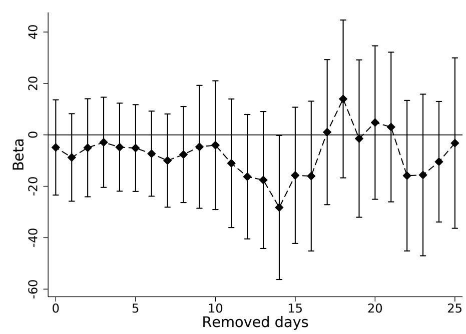
(b) Mother's leave uptake in days



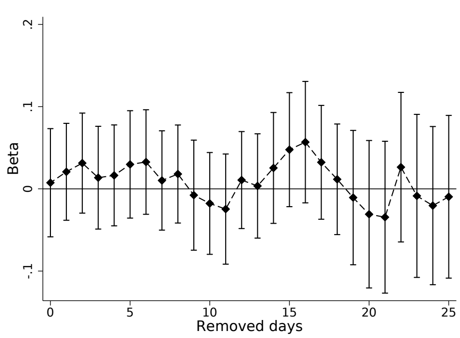
(c) Mean leangth of spell, fathers.



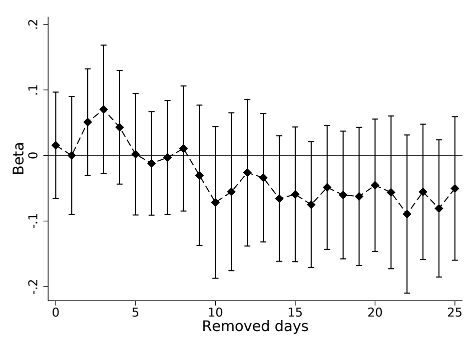
(d) Mean length of spell, mothers.



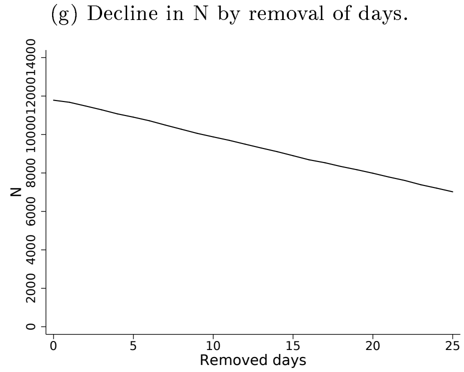
(e) Father's probability of taking at least 50 work days of leave.



(f) Longer, less compensated leave.



(g) Decline in N by removal of days.



Note: N=9 110. The sample is opposite-sex couples with children born in 2008, either between May 1-June 17 (control) or July 14-August 31 (treatment). Couples must have co-resided as of January 1 2007, and the mother must be registered with earned income in 2007, and siblings (if any) must be born at least 16 months before/after the focal child.

Table A.1: Descriptive statistics of outcome variables. Main sample, sociodemographic outcomes.

	Mean	SD	P25	Median	P75	Min	Max
Joint characteristics							
Additional sibling							
N child 1y	0,00	0,00	0,00	0,00	0,00	0,00	0,00
N child 2y	0,08	0,27	0,00	0,00	0,00	0,00	2,00
N child 3y	0,22	0,42	0,00	0,00	0,00	0,00	2,00
N child 4y	0,31	0,49	0,00	0,00	1,00	0,00	2,00
N child 5y	0,37	0,54	0,00	0,00	1,00	0,00	3,00
Any child 1y	0,00	0,00	0,00	0,00	0,00	0,00	0,00
Any child 2y	0,07	0,26	0,00	0,00	0,00	0,00	1,00
Any child 3y	0,21	0,41	0,00	0,00	0,00	0,00	1,00
Any child 4y	0,30	0,46	0,00	0,00	1,00	0,00	1,00
Any child 5y	0,34	0,48	0,00	0,00	1,00	0,00	1,00
Union intact when							
Child 1y	0,98	0,15	1,00	1,00	1,00	0,00	1,00
Child 2y	0,96	0,20	1,00	1,00	1,00	0,00	1,00
Child 3y	0,94	0,24	1,00	1,00	1,00	0,00	1,00
Child 4y	0,92	0,27	1,00	1,00	1,00	0,00	1,00
Child 5y	0,90	0,31	1,00	1,00	1,00	0,00	1,00
Mother's share earnings							
Child 1y	0,38	0,19	0,27	0,38	0,47	0,00	1,00
Child 2 y	0,39	0,19	0,29	0,40	0,48	0,00	1,00
Child 3 y	0,39	0,19	0,29	0,40	0,48	0,00	1,00
Child 4 y	0,39	0,19	0,29	0,40	0,49	0,00	1,00
Child 5 y	0,40	0,19	0,30	0,40	0,49	0,00	1,00
Father characteristics							
Working when							
Child 1y	0,96	0,20	1,00	1,00	1,00	0,00	1,00
Child 2 y	0,96	0,20	1,00	1,00	1,00	0,00	1,00
Child 3 y	0,96	0,20	1,00	1,00	1,00	0,00	1,00
Child 4 y	0,95	0,21	1,00	1,00	1,00	0,00	1,00
Child 5 y	0,95	0,21	1,00	1,00	1,00	0,00	1,00
Log earnings when							
Child 1y	12,76	1,94	12,83	13,05	13,33	0,00	16,01
Child 2 y	12,80	2,02	12,89	13,11	13,40	0,00	16,09
Child 3 y	12,83	2,12	12,95	13,17	13,45	0,00	16,01
Child 4 y	12,85	2,23	12,99	13,22	13,51	0,00	16,22
Child 5 y	12,86	2,34	13,03	13,27	13,56	0,00	16,85
Mother characteristics							
Working when							
Child 1y	0,91	0,28	1,00	1,00	1,00	0,00	1,00
Child 2 y	0,90	0,30	1,00	1,00	1,00	0,00	1,00
Child 3 y	0,91	0,29	1,00	1,00	1,00	0,00	1,00
Child 4 y	0,91	0,28	1,00	1,00	1,00	0,00	1,00
Child 5 y	0,91	0,29	1,00	1,00	1,00	0,00	1,00
Log earnings when							
Child 1y	12,12	2,07	12,17	12,58	12,83	0,00	14,60
Child 2 y	12,06	2,58	12,33	12,73	12,96	0,00	15,32
Child 3 y	12,17	2,48	12,44	12,79	13,03	0,00	14,64
Child 4 y	12,19	2,61	12,49	12,84	13,08	0,00	15,82
Child 5 y	12,21	2,72	12,55	12,89	13,13	0,00	15,20

Note: N=9 757. The sample is opposite-sex couples with children born in 2009, either between May 1-June 17 (control) or July 14-August 31 (treatment). Couples must have co-resided as of January 1 2008, and the mother must be registered with earned income in 2008.

Table A.2: Descriptive statistics of outcome variables. Parental leave sample.

	Mean	SD	P25	Median	P75	Min	Max
Father characteristics							
Leavedays	40,03	37,79	18,00	36,00	55,20	0,00	528,00
Leavedays capped	39,86	36,37	18,00	36,00	55,20	0,00	280,00
Takes compensated leave	0,78	0,42	1,00	1,00	1,00	0,00	1,00
Leave >= 50 days	0,37	0,48	0,00	0,00	1,00	0,00	1,00
Number of spells	1,10	1,51	1,00	1,00	1,00	0,00	44,00
Mean duration of spells	34,40	34,45	10,00	30,42	50,00	0,00	528,00
Uses time account	0,09	0,29	0,00	0,00	0,00	0,00	1,00
80% compensation	0,50	0,50	0,00	0,00	1,00	0,00	1,00
Mother characteristics							
Leavedays	218,52	102,07	180,00	258,00	282,00	0,00	796,80
Leavedays capped	209,99	89,89	180,00	258,00	280,00	0,00	280,00
Takes compensated leave	0,89	0,32	1,00	1,00	1,00	0,00	1,00
Number of spells	0,94	0,42	1,00	1,00	1,00	0,00	4,00
Mean duration of spells	212,36	102,59	176,00	246,00	280,80	0,00	796,80
Uses time account	0,02	0,12	0,00	0,00	0,00	0,00	1,00

Note: N=9 516. The sample is opposite-sex couples with children born in 2009, either between May 1-June 17 (control) or July 14-August 31 (treatment). Couples must have co-resided as of January 1 2008, and the mother must be registered with earned income in 2008, and siblings (if any) must be born at least 16 months before/after the focal child.

Table A.3: Reform effects on leave uptake and outcomes. Restricted sample. OLS/LPM estimates from Regression Discontinuity models.

	No controls			Full controls		
	Est	(SE)		Est	(SE)	
A: LEAVE UPTAKE FATHERS						
Number of days	15,53	(9,15)		16,76	(8,26)	†
Use time account	-0,08	(0,06)	†	-0,07	(0,05)	
Takes leave	0,02	(0,12)		0,03	(0,10)	
Mean duration of spell	8,63	(8,65)		10,48	(8,44)	
Number of spells	0,26	(0,35)		0,19	(0,30)	
80% compensation	-0,12	(0,11)		-0,13	(0,10)	
B: LEAVE UPTAKE MOTHERS						
Number of days	-26,36	(17,44)		-26,74	(19,22)	
Use time account	-0,04	(0,02)	†	-0,03	(0,02)	
Takes leave	-0,05	(0,07)		-0,05	(0,07)	
Mean duration of spell	-28,17	(16,92)	†	-28,71	(17,61)	
Number of spells	-0,06	(0,09)		-0,04	(0,10)	
C: DEMOGRAPHIC OUTCOMES						
Intact union ch. 5 y	-0,04	(0,05)		-0,04	(0,05)	
At least one younger sibling 5 y	-0,08	(0,12)		-0,08	(0,10)	
N younger siblings 5 y	-0,07	(0,11)		-0,08	(0,10)	
D: EARNINGS OUTCOMES						
Mothers' share ch. 5 y	0,02	(0,05)		0,02	(0,04)	
Father working ch. 5 y	-0,04	(0,06)		-0,03	(0,05)	
Father ln(earn.) ch. 5 y	-0,39	(0,78)		-0,31	(0,72)	
Mother working ch. 5 y	0,01	(0,06)		0,01	(0,06)	
Mother ln(earn.) ch. 5 y	-0,21	(0,58)		-0,12	(0,46)	

Note: N=3 358 for the parental leave sample and 3 450 for the sociodemographic sample. The sample is opposite-sex couples with children born in 2009, either between June 1-June 17 (control) or July 14-July31 (treatment). Couples must have co-resided as of January 1 2008, and the mother must be registered with earned income in 2008. In the parental leave sample, siblings (if any) must be born at least 16 months before/after the focal child. *** $p<0.001$, ** $p<0.01$, * $p<0.05$, † $p<0.1$.

Table A.4: Reform effects on leave uptake and outcomes. Extended sample. OLS/LPM estimates from Regression Discontinuity models.

	No controls			Full controls		
	Est	SE		Est	SE	
A: LEAVE UPTAKE FATHERS						
Number of days	14,83	(2,27)	***	15,06	(2,06)	***
Use time account	0,02	(0,02)		0,02	(0,02)	
Takes leave	0,02	(0,03)		0,01	(0,02)	
Takes >= 50 days leave	0,53	(0,03)	***	0,53	(0,03)	***
Mean duration of spell	11,51	(1,95)	***	11,74	(2,03)	***
Number of spells	0,03	(0,11)		0,03	(0,11)	
80% compensation	-0,07	(0,03)	*	-0,07	(0,03)	*
B: LEAVE UPTAKE MOTHERS						
Number of days	-14,77	(5,11)	**	-14,75	(5,17)	**
Use time account	0,00	(0,01)		0,00	(0,01)	
Takes leave	-0,01	(0,02)		-0,01	(0,02)	
Mean duration of spell	-15,50	(6,53)	*	-15,72	(6,62)	*
Number of spells	-0,03	(0,02)		-0,03	(0,02)	
C: DEMOGRAPHIC OUTCOMES						
Intact union ch. 5 y	0,00	(0,02)		0,00	(0,02)	
At least one younger sibling 5 y	0,00	(0,03)		-0,02	(0,03)	
N younger siblings 5 y	0,00	(0,04)		-0,02	(0,03)	
D: EARNINGS OUTCOMES						
Mothers' share ch. 5 y	0,02	(0,01)		0,01	(0,01)	
Father working ch. 5 y	-0,02	(0,01)		-0,01	(0,01)	
Father ln(earn.) ch. 5 y	-0,17	(0,15)		-0,14	(0,14)	
Mother working ch. 5 y	0,00	(0,02)		0,00	(0,02)	
Mother ln(earn.) ch. 5 y	0,02	(0,18)		0,05	(0,17)	

Note: N=20 872 in the parental leave sample and 21 421 in the sociodemographic sample. The sample is opposite-sex couples with children born in 2009, either between March 1-June 17 (control) or July 14- October 31 (treatment). Couples must have co-resided as of January 1 2008, and the mother must be registered with earned income in 2008. In the parental leave sample, siblings (if any) must be born at least 16 months before/after the focal child.. *** p<0.001, ** p<0.01, * p<0.05,† p<0.1.

Table A.5: Reform effects on leave uptake and outcomes. Simpler specifications OLS/LPM estimates from Regression Discontinuity models.

	DUMMY			LINEAR			SQUARE		
	Est	SE		Est	SE		Est	SE	
A: LEAVE UPTAKE FATHERS									
Number of days	13,95	(0,66)	***	15,39	(1,51)	***	15,43	(1,51)	***
Takes leave	0,01	(0,01)		0,00	(0,02)		0,00	(0,02)	
Takes >= 50 days leave	0,51	(0,01)	***	0,54	(0,02)	***	0,54	(0,02)	***
80% compensation	-0,10	(0,01)	***	-0,07	(0,02)	***	-0,07	(0,02)	***
B: LEAVE UPTAKE MOTHERS									
Number of days	-15,64	(1,79)	***	-16,74	(3,17)	***	-16,55	(3,11)	***
Takes leave	-0,01	(0,01)		-0,02	(0,01)	*	-0,02	(0,01)	*
C: DEMOGRAPHIC OUTCOMES									
Intact union ch. 5 y	-0,01	(0,01)		0,00	(0,01)		0,00	(0,01)	
At least one younger sibling 5 y	0,01	(0,01)		0,02	(0,02)		0,02	(0,02)	
D: EARNINGS OUTCOMES									
Mothers' share ch. 5 y	0,01	(0,00)		0,01	(0,01)		0,01	(0,01)	
Father working ch. 5 y	0,00	(0,00)		-0,01	(0,01)		-0,01	(0,01)	
Father ln(earn.) ch. 5 y	-0,03	(0,05)		-0,06	(0,10)		-0,07	(0,10)	
Mother working ch. 5 y	0,00	(0,01)		0,00	(0,01)		0,00	(0,01)	
Mother ln(earn.) ch. 5 y	-0,01	(0,05)		-0,01	(0,09)		-0,01	(0,09)	

Note: N=9 516 for the parental leave sample and 9 757 for the sociodemographic sample. The samples are opposite-sex couples with children born in 2009, either between May 1-June 17 (control) or July 14-August 31 (treatment). Couples must have co-resided as of January 1 2008, and the mother must be registered with earned income in 2008. In the parental leave sample, siblings (if any) must be born at least 16 months before/after the focal child. *** p<0.001, ** p<0.01, * p<0.05, † p<0.1.

Table A.6: Descriptive statistics of outcome variables. Placebo analysis sample.

	Mean	SD	P25	Median	P75	Min	Max
A: LEAVE UPTAKE FATHERS							
Number of days	33,12	32,26	15,00	36,00	36,00	0,00	280,00
Use time account	0,06	0,24	0,00	0,00	0,00	0,00	1,00
Takes leave	0,77	0,42	1,00	1,00	1,00	0,00	1,00
Takes >= 50 days leave	0,12	0,32	0,00	0,00	0,00	0,00	1,00
Mean duration of spell	29,08	30,63	7,20	30,00	36,00	0,00	455,00
Number of spells	1,08	1,52	1,00	1,00	1,00	0,00	30,00
80% compensation	0,56	0,50	0,00	1,00	1,00	0,00	1,00
B: LEAVE UPTAKE MOTHERS							
Number of days	0,01	0,11	0,00	0,00	0,00	0,00	1,00
Use time account	217,13	92,81	189,00	276,00	280,00	0,00	280,00
Takes leave	0,88	0,33	1,00	1,00	1,00	0,00	1,00
Mean duration of spell	222,59	108,06	185,00	272,40	288,00	0,00	1304,40
Number of spells	0,93	0,53	1,00	1,00	1,00	0,00	31,00
C: DEMOGRAPHIC OUTCOMES							
Intact union ch. 5 y	0,90	0,30	1,00	1,00	1,00	0,00	1,00
At least one younger sibling 5 y	0,29	0,45	0,00	0,00	1,00	0,00	1,00
N younger siblings 5 y	0,31	0,50	0,00	0,00	1,00	0,00	3,00
D: EARNINGS OUTCOMES							
Mothers' share ch. 5 y	0,39	0,19	0,29	0,40	0,48	0,00	1,00
Father working ch. 5 y	0,95	0,21	1,00	1,00	1,00	0,00	1,00
Father ln(earn.) ch. 5 y	12,86	2,27	13,01	13,24	13,52	0,00	15,90
Mother working ch. 5 y	0,91	0,29	1,00	1,00	1,00	0,00	1,00
Mother ln(earn.) ch. 5 y	12,16	2,70	12,51	12,85	13,09	0,00	15,62

Note: N=9 110 for parental leave outcomes and 9 320 for sociodemographic outcomes. The sample is opposite-sex couples with children born in 2008, either between May 1-June 17 or July 15-August 31. Couples must have co-resided as of January 1 2007, and the mother must be registered with earned income in 2007. For parental leave outcomes, siblings (if any) must be born at least 16 months before/after the focal child.

Table A.7: Mean differences by treatment status. Outcome variables. Parental leave placebo sample (Panel A) and sociodemographic placebo sample (Panel B)

Panel A: Parental leave outcomes			
	Post	Pre	Post - Pre
Father's days of leave (org.)	32.84	33.44	-0.60
Father's days of leave	32.83	33.38	-0.55
Father takes leave	0.77	0.77	0.00
Father takes >=10 weeks	0.11	0.12	-0.01
Father N leave spells	1.05	1.10	-0.05
Father mean duration spell	29.00	29.15	-0.15
Father uses time account	0.06	0.06	0.00
Father 80% compensation	0.53	0.58	-0.05***
Mother's days of leave (org.)	227.09	230.17	-3.08
Mother's days of leave	216.39	217.84	-1.46
Mother takes leave	0.88	0.87	0.01
Mother N leave spells	0.94	0.93	0.02
Mother mean duration spell	221.04	224.07	-3.03
Mother uses time account	0.01	0.01	0.00
Observations	9110		
Panel B: Sociodemographic outcomes			
	Post	Pre	Post - Pre
Union intact 2y	0.98	0.97	0.00
Union intact 5y	0.90	0.90	0.00
Mother's share 2y	0.38	0.38	-0.00
Mother's share 5y	0.39	0.39	-0.00
Father working 2y	0.96	0.96	0.00
Father working 5y	0.96	0.95	0.00
Father ln(earn) 2y	12.80	12.80	-0.00
Father ln(earn) 5y	12.89	12.83	0.06
Mother working 2y	0.91	0.90	0.01
Mother working 5y	0.91	0.91	-0.00
Mother ln(earn) 2y	12.05	11.98	0.07
Mother ln(earn) 5y	12.15	12.17	-0.01
N younger sibs 5y	0.30	0.31	-0.01
Has younger sib 5y	0.28	0.29	-0.01
Observations	9320		

Note: N=9 110 for parental leave outcomes and 9 320 for sociodemographic outcomes. The sample is opposite-sex couples with children born in 2008, either between May 1-June 17 (control) or July 14-August 31 (treatment). Couples must have co-resided as of January 1 2007, and the mother must be registered with earned income in 2007. In the parental leave sample, siblings (if any) must be born at least 16 months before/after the focal child.

Table A.8: Reform effects on leave uptake and outcomes. Placebo sample. OLS/LPM estimates from Regression Discontinuity models.

	No controls			Full controls		
	Est	(SE)		Est	(SE)	
A: LEAVE UPTAKE FATHERS						
Number of days	2,25	(3,75)		3,81	(3,51)	
Use time account	-0,04	(0,03)	†	-0,04	(0,03)	†
Takes leave	-0,02	(0,05)		0,01	(0,05)	
Takes >= 50 days leave	0,03	(0,03)		0,02	(0,03)	
Mean duration of spell	2,11	(3,47)		3,30	(3,27)	
Number of spells	-0,09	(0,17)		-0,07	(0,16)	
80% compensation	-0,07	(0,05)		-0,06	(0,05)	
B: LEAVE UPTAKE MOTHERS						
Number of days	-19,84	(10,88)	*	-13,42	(10,00)	
Use time account	0,00	(0,01)		0,00	(0,01)	
Takes leave	-0,06	(0,04)	†	-0,04	(0,04)	
Mean duration of spell	-28,25	(14,29)	*	-20,36	(13,26)	
Number of spells	-0,06	(0,05)		-0,03	(0,04)	
C: DEMOGRAPHIC OUTCOMES						
Intact union ch. 5 y	0,03	(0,04)		0,03	(0,04)	
At least one younger sibling 5 y	-0,03	(0,04)		-0,04	(0,04)	
N younger siblings 5 y	-0,05	(0,05)		-0,05	(0,04)	
D: EARNINGS OUTCOMES						
Mothers' share ch. 5 y	0,01	(0,02)		0,02	(0,02)	
Father working ch. 5 y	0,01	(0,02)		0,01	(0,02)	
Father ln(earn.) ch. 5 y	-0,01	(0,22)		0,03	(0,19)	
Mother working ch. 5 y	0,03	(0,03)		0,04	(0,03)	
Mother ln(earn.) ch. 5 y	0,19	(0,31)		0,30	(0,30)	

Note: N=9 110 for parental leave outcomes and 9 320 for sociodemographic outcomes. The sample is opposite-sex couples with children born in 2008, either between May 1-June 17 (control) or July 14-August 31 (treatment). Couples must have co-resided as of January 1 2007, and the mother must be registered with earned income in 2007. In the parental leave sample, siblings (if any) must be born at least 16 months before/after the focal child.

Table A.9: Mean differences by treatment status, placebo sample. Balancing tests on pre-reform characteristics. Parental leave sample (Panel A) and sociodemographic sample (Panel B)

Panel A: Parental leave placebo sample			
	Post	Pre	Post - Pre
Married	0.47	0.48	-0.01
Parity	2.05	2.04	0.01
Mother's age	32.37	32.53	-0.16
Mother's earnings	289564.33	289402.41	161.92
Mother basic educ.	0.12	0.12	0.00
Mother HS educ.	0.28	0.29	-0.01
Mother higher educ., lower degr.	0.45	0.45	-0.00
Mother higher educ., higher degr.	0.13	0.13	-0.00
Mother missing educ.	0.02	0.02	0.00
Mother student	0.10	0.08	0.01*
Father's age	35.20	35.13	0.07
Father's earnings	454011.31	448712.07	5299.25
Father basic educ.	0.17	0.16	0.01
Father HS educ.	0.40	0.41	-0.01
Father higher educ., lower degr.	0.28	0.27	0.00
Father higher educ., higher degr.	0.14	0.15	-0.00
Father missing educ.	0.01	0.02	-0.00
Father student	0.06	0.06	0.00
Observations	9110		
Panel B: Sociodemographic placebo sample			
	Post	Pre	Post - Pre
Married	0.47	0.48	-0.01
Parity	2.06	2.05	0.01
Mother's age	32.35	32.48	-0.14
Mother's earnings	288807.94	288196.87	611.07
Mother basic educ.	0.12	0.12	0.00
Mother HS educ.	0.29	0.29	-0.01
Mother higher educ., lower degr.	0.45	0.44	0.00
Mother higher educ., higher degr.	0.12	0.12	-0.00
Mother missing educ.	0.02	0.02	0.00
Mother student	0.10	0.08	0.01*
Father's age	35.17	35.10	0.07
Father's earnings	452996.61	447523.37	5473.24
Father basic educ.	0.17	0.16	0.01
Father HS educ.	0.40	0.41	-0.01
Father higher educ., lower degr.	0.28	0.27	0.01
Father higher educ., higher degr.	0.14	0.14	-0.00
Father missing educ.	0.01	0.02	-0.00
Father student	0.06	0.06	-0.00
Observations	9320		

Note: Note: N=9 110 for parental leave outcomes and 9 320 for sociodemographic outcomes. The sample is opposite-sex couples with children born in 2008, either between May 1-June 17 (control) or July 14-August 31 (treatment). Couples must have co-resided as of January 1 2007, and the mother must be registered with earned income in 2007. In the parental leave sample, siblings (if any) must be born at least 16 months before/after the focal child.

Appendix II: Details on data and sample for parental leave outcomes

Parental leave data are obtained from the FD Trygd Database (Akselsen et al., 2007), which contains information on receipt of a range of social transfers. FD Trygd consists of "spells" of transfer reciept. One parent's leave after one birth can be composed of more than one spell. Spells are registered to parents, and must be linked to children by assumptions. With a maximum leave length of 56 (46) weeks at 80 (100) percent compensation, and few alternatives to parental care for children under 1 year in Norway, the vast majority of leave is taken within the first 1.5 year of the child's life (Fougner, 2012). We assign leave spells to a child if the following two criteria are met:

1. The leave starts no earlier than the birth date (for fathers) or no earlier than [three] weeks before the birth date (for mothers). Fathers cannot take leave before a child is born. Norwegian expectant mothers are mandated by law to start their parental leave no later than three weeks before their due date. Births are medically induced 12 days past due date the latest (`https://helsedirektoratet.no/retningslinjer/svangerskapsomsorgen`). Leaving some time for the birth to happen, the maximum duration between leave start and birth date should hence be five weeks.
2. The leave starts no later than 13 months and three weeks after the child is born.

The second restriction assigns leave spells accurately if children with a sibling born within 16 months of own birth are excluded from the sample. Hence, analysis of parental leave outcomes are done on a sample that is strictly speaking endogenously conditioned with respect to fertility spacing, and we estimate effects only for the subsample of children with no closely spaced sibling. Children with closely spaced siblings differ systematically from children with siblings born further apart, faring somewhat worse on a range of health outcomes (Conde-Agudelo et al., 2006).

It is reassuring that we find no empirical evidence of endogeneity with respect to fertility: Our estimates show that the reform does not affect fertility, and robustsness tests show no differences between the treatment and control groups with respect to pre-reform fertility. Still, we perform analysis on sociodemographic outcomes in a separate sample that is not restricted by this endogenous conditions.